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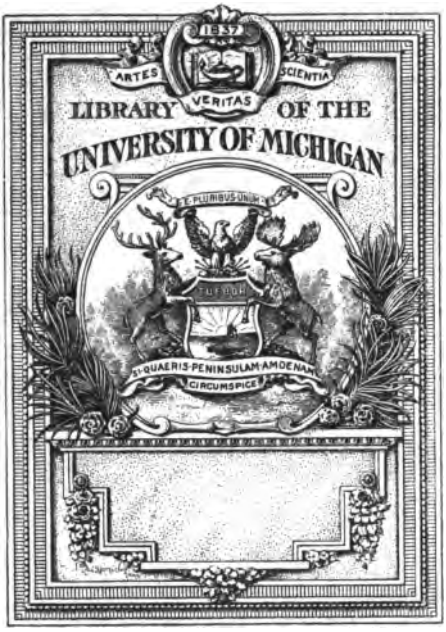
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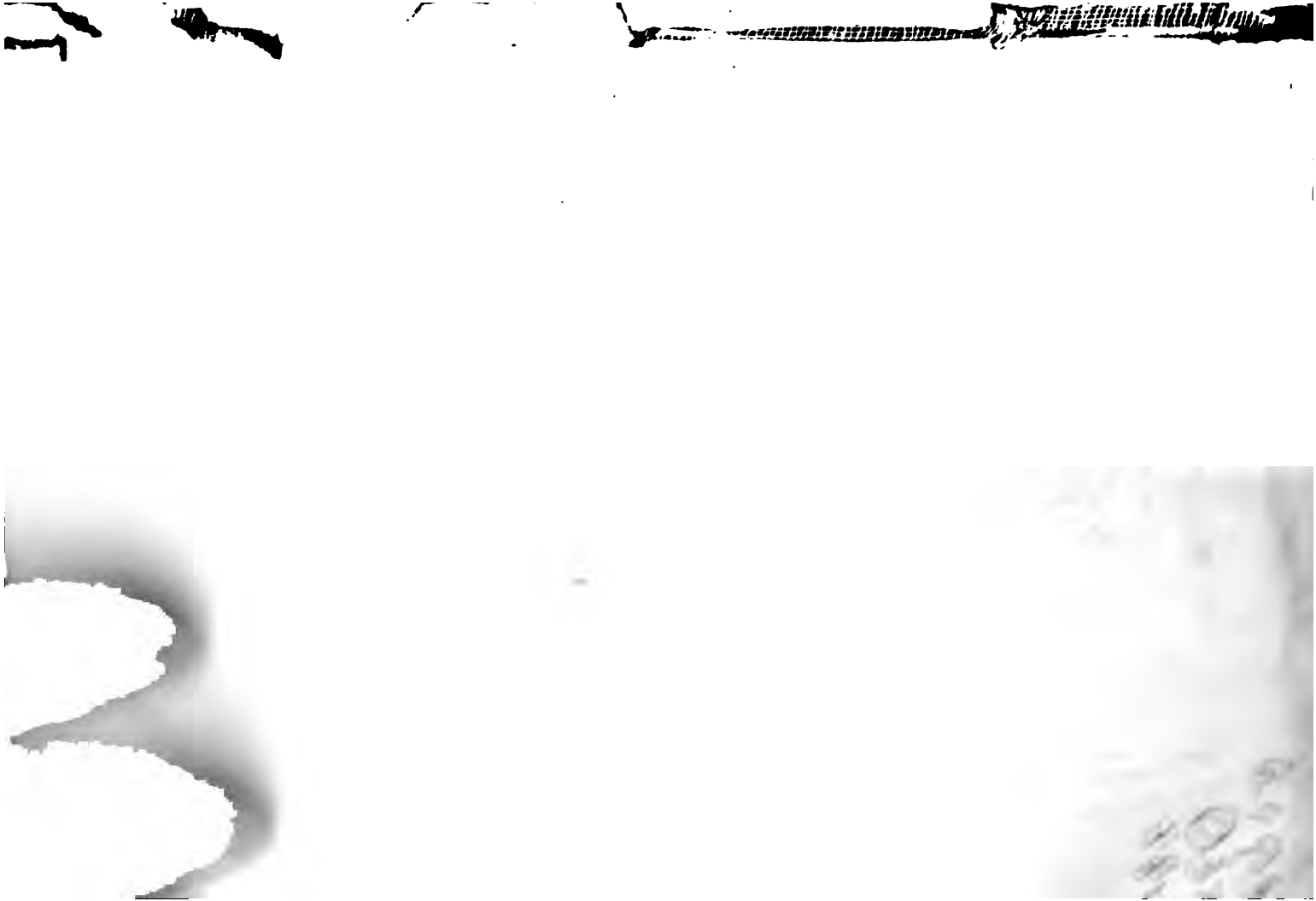

Yours Truly
W. J. Conklin.

TRANSACTIONS

OF THE

123092

FORTY-SIXTH ANNUAL MEETING

OF THE

OHIO STATE

MEDICAL SOCIETY,

HELD AT

SANDUSKY

JUNE 17, 18 and 19, 1891.

TOLEDO, OHIO:
The Blade Printing & Paper Co.,
1891.

Officers for 1891-92.

G. A. Collamore, M. D., Toledo ---------- President.
X. C. Scott, M. D., Cleveland ---------- Vice-President.
A. J. Gawne, M. D., Sandusky ---------- Vice-President.
A. R. Baker, M. D., Cleveland ---------- Vice-President.
F. D. Bain, M. D , Kenton ---------- Vice-President.
T. V. Fitzpatrick, M. D., Cincinnati ---------- Secretary.
Auguste Rhu, M. D., Marion ---------- Assistant Secretary.
T. W. Jones, M. D., Columbus ---------- Treasurer and Librarian.

Standing Committees.

FINANCE.

H. J. Herrick, M. D., E. Sinnett, M. D., W. S. Hough, M. D.,
D. P. Allen, M. D., W. T. Corlett, M. D.

ETHICS.

S. S. Thorn, M. D., H. D. Hinckley, M. D., H. S. Upson, M. D.,
W. J. Scott, M. D., C. L. Van Pelt, M. D.

PUBLICATION.

E. S. McKee, M. D., C. A. L. Reed, M. D., J. F. Baldwin, M. D.,
A. R. Baker, M. D., J. Ransohoff, M. D.,
and G. A. Collamore, M. D., T. V. Fitzpatrick, M. D., *ex officiis.*

LEGISLATION.

D. J. Snyder, M. D., S. S. Thorn, M. D., S. Loving, M. D.,
D. N. Kinsman, M. D., D. C. Wilson, M. D.

ADMISSIONS.

A. F. House, M. D., C. F. Clark, M. D., D. J. Merriman, M. D.,
C. P. Landon, M. D., H. M. Brown, M. D.

OHIO STATE MEDICAL SOCIETY.

At the Seventeenth Annual Session of the Ohio State Medical Society, the following resolution was adopted:

Resolved, That the publication of any address, prize essay, or other paper, by this Society, is no evidence of its endorsement of the sentiments therein contained.

FORTY-SIXTH ANNUAL MEETING

OF THE

OHIO STATE MEDICAL SOCIETY

HELD AT

SANDUSKY, JUNE 17, 18 AND 19, 1891.

The Society met in Odd Fellows Hall at two o'clock P. M., June 17, 1891, President Conklin in the chair.

Prayer was offered by the Rev. Mr. Graham.

The report of the Committee on Arrangements was made by Dr. S. S. Thorn.

The Secretary submitted the following report:

By last report the membership of the Society was 536. At the last meeting 29 new members were admitted and three have since been restored to membership.

On the other hand there were seven deaths reported and subsequently 30 were dropped for non-payment of dues, leaving the membership 531, a loss of five.

The deaths of the following members have been reported during the year:

A. C. Belden, Akron; F. W. Brayton, Carey; A. B. Carpenter, Cleveland; Howard Culbertson, Zanesville; B.

W. Davis, Marion; I. L. Drake, Lebanon; W. H. Emery, Mt. Sterling; S. P. Grill, Orrville; E. W. Howard, Akron; Homer Johnson, Oberlin; A. P. Knowlton, Berea; I. F. Markle, Mifflin; A. R. Smart, Toledo; W. J. Underwood, Akron; F. J. Weed, Cleveland.

EXPENSES.

Postage, Postal Cards and Envelopes	$30 78
Printing and Folding	18 56
Stationery	50
	$49 84

The only auxiliary society that has fallen out during the year is the Gallia Co. Med. Society. Several others are in feeble condition, but are continued on the list in the hope of future revival.

All auxiliary societies have forwarded the annual catalogue except Brown Co., Butler Co. and South Eastern.

At the request of the Committee on Organization of County Societies, etc., the report of that committee with the following circular was forwarded:

OHIO STATE MEDICAL SOCIETY,
SECRETARY'S OFFICE,
TOLEDO, OHIO, July 12, 1890.

TO THE SECRETARIES OF MEDICAL SOCIETIES WHICH ARE AUXILIARY TO THE STATE SOCIETY:

Dear Sirs:—The following report is forwarded to you with the request that you bring the subject before your respective societies for their consideration, and inform me as soon as possible of their opinion as to the propriety and advisability of the plan proposed.

Very respectfully,

G. A. COLLAMORE,
Secretary.

Responses were received from 22 of the auxiliary societies—all being favorable to the proposed plan.

These responses have been forwarded to the committee for its use.

More than two thousand programs have been distributed to the medical profession of the State, and a good attendance is expected at this meeting.

By request, the President's address has been transferred on the program to the evening session. As this involves a suspension of the By-laws, it is hoped that the Society will approve the program.

Applications to become auxiliary have been received from the "Fayette Co. Med. Society" and the "Hancock Co. Med. Society."

The following communications have been received:

OHIO PHARMACEUTICAL ASSOCIATION,
CLEVELAND, O., June 6th, 1891.

G. A. Collamore, M. D., Sec'y Ohio State Medical Society, Toledo, Ohio.

DEAR SIR: The enclosed is a copy of a resolution adopted at the last annual meeting of the Ohio State Pharmaceutical Association held at Toledo, Ohio, June 10th, 11th and 12th, 1890:

WHEREAS, The Ohio State Medical Society at its ann ual meeting in Columbus, June 5, 1890, adopted by a un animous vote, resolutions endorsing the present pharmacy law of this State, deploring the action of the Ohio legislature in the attempt to amend it so as to permit all physicians and persons having served a specified time in any drug store in the United States to engage in pharmacy in this State without examination and without registration. And also the proposition to repeal the pharmacy law enacted in 1884. Therefore be it

Resolved, That the Ohio Pharmaceutical Association expresses its very high appreciation of this action on the part of the State Medical Society, coming as it does from a body of men so eminently able to understand the necessity of having pharmacists who, to some extent at least, are qualified to perform their important duties.

Resolved, That this Association pledges its encouragement and assistance individually and collectively to the Ohio Medical Society in any attempt it shall make hereafter as an organization to secure proper legislation regulating the practice of medicine in the State of Ohio.

Resolved, That a copy of these resolutions be transmitted to the State Medical Society, through its Secretary, Dr. G. A. Collamore, Toledo, Ohio.

Very respectfully,

LEWIS C. HOPP,
Permanent Sec'y.

AMERICAN MEDICAL ASSOCIATION,
Philadelphia, 1889.

DEAR DOCTOR: At our recent session in Newport, the Committee on Uniform Medical Legislation in the United States presented the following report, which was unanimously adopted, and a copy was directed to be sent to the Secretary of each State Medical Society, with the recommendation that each society exert itself to secure the enactment of a law embodying the provisions of the report:

"That, in our judgment, the best interests of the public will be subserved by the enactment of efficient medical legislation in every State in the Union.

"That for the convenience of the profession and the stimulating effect on medical education in this country, it is advisable to secure uniformity of legislation in the essential features of all Medical Practice Acts.

"This Committee therefore begs to recommend as follows—that in future medical legislation the essential features of the enactment be as follows:

"That all persons commencing the practice of medicine in any of its branches shall possess a license from the State Board of Medical Examiners.

"That all candidates for a license shall submit satisfactory documentary evidence that he or she is a graduate in medicine of a medical institution in good standing with the said Board, and having a curriculum possessing at least the following requirements:

"First.—An entrance examination to test the student's fitness to become a practitioner. This examination shall include at least an examination in English grammar, composition, geography, history, arithmetic, algebra, physics, and the natural sciences; together with at least one of the following languages: Latin, French or German; provided, however, that graduates of reputable colleges may be exempt from said examination.

"Second.—Before granting a degree of M. D. or M. B., candidates for the same shall have attended at least three full and regular courses of medicine of not less than six months' duration each.

"All candidates for a license shall undergo an examination by the said Board of Medical Examiners upon the branches usually taught in medical colleges. Said examination shall be both scientific and practical, but of sufficient severity to test the candidate's fitness to practice medicine and surgery.

"Said Board of Medical Examiners shall issue a license to only such persons undergoing an examination as may be deemed suitable to practice medicine. Said Board may refuse or revoke a license for the following named causes, to wit: chronic and persistent inebriety, criminal abortion, or gross unprofessional conduct.

"All licenses shall be recorded and made a matter of public record with the County Clerk, or Clerk of District Court, in the county wherein resides said person.

"Said Board of Medical Examiners shall be appointed by the Governor, for a period not exceeding five years, the members thereof to be chosen from among the reputable practitioners ot medicine in the State of not less than five years' residence."

Please acknowledge the receipt of this, and after your Society has acted, please inform me what action they have taken.

Yours very truly,

WM. B. ATKINSON.

WEST VIRGINIA MEDICAL ASSOCIATION,
PARKERSBURG, W. VA., Dec. 4, 1890.

To the Medical Society of Ohio:

At the annual meeting of the State Medical Society of West Virginia, June 11th, 1890, a committee was appointed to memorialize the State Societies of adjoining States and the American Medical Association, upon the subject of the relations of contract surgeons of the different railroad systems to the general profession, and to ask these societies to consider how far the rules adopted by railroad corporations for the government of the surgeons in their service, infringe upon the rights of the profession at large, as set forth in the code of ethics of the American Medical Association. It is well known that large bodies of men are in the employ of these corporations; that these men live in widely scattered communities, and that these corporations have established systems of contract surgeons to attend to employes and passengers injured by accidents occurring while in their employ, or care as passengers. It is also well known that these corporations have adopted rules for the government of these surgeons and of those injured, directing that these surgeons assume entire charge of such employes or passengers, when injured, regardless of the rights of any outside medical man who may have been summoned and is in attendance upon such persons prior to the arrival of the company's surgeon, even if in addition the physician first in attendance be the family and personal attendant of the party injured, and in most cases have served a notice to this effect on the general profession. For instance: Section 4 of General Order No. 2, of the Baltimore & Ohio Railroad Relief Department, says: "Upon the arrival of the company's surgeon and taking charge of the patient, the called surgeon, if one has been called, shall be distinctly notified by the company's surgeon, and by the officer in charge, that the company will be no longer responsible for his attendance or service, and that they are no longer required." In Section 5 of the same company's instructions, it says: "Company's surgeons upon arriving at the place of accident will take exclusive charge of the case, and entirely relieve any surgeon, other than the one under contract with the company, from further care or attendance. The called surgeon shall be distinctly notified by the company's surgeon and the officer in charge, that the company will be no longer responsible for his attendance or services. Should the patient prefer the services of a surgeon other than the one provided by the company, it must be distinctly understood by the patient and the said surgeon, that neither the company nor the Relief Association will be responsible for the services rendered."

To the majority of the State Society this appears to place the physicians in direct conflict with Section 6 of Article 5 of the Code of Ethics, and causes infringements upon the rights of physicians first called, who charitably respond to such calls, while knowing the rules of the company, because of the urgency of the case.

Again, in the establishment of these systems of contract surgeons, we believe the practice of accepting passes as compensation, or in lieu of the regular fees customary to the profession, is detrimental to the best interests of the profession, by lowering the standard of the values of surgical services; and further demoralizing, because it gives to these wealthy corporations services at far less rates than the profession charge to individuals for like services; and further that these corporations come under that part of Section 3 of Article 1, relating to the duties of the profession to the public, and *vice versa*, which exempts

institutions endowed by rich individuals, societies for mutual benefit, for the insurance of lives, &c., &c.. from the privileges set forth in Section 1 and Section 2 of the same article.

Further, it seems to your memorialists, that if members of the profession are left to make contracts to furnish an unlimited service of the kind required, for passes. and in some cases small fixed money payments, with these large corporations, without affecting their ethical standing, all stigma of unprofessional or unethical conduct should be removed from those of the profession who contract with private individuals to furnish medical or surgical services, including medicines, by the year or month, at fixed sums.

In conclusion, we hope your Society will consider this subject, and if agreeing with us that this is an evil, join us in bringing this to the American Medical Association for it to decide what action, if any, is proper under the circumstances.

In behalf of the above Committee, the foregoing is respectfully submitted to your Society for their consideration.

WESLEY H. SHARP, M. D.,
Chairman of Committee.

Respectfully submitted,

G. A. COLLAMORE, *Sec'y.*

Dr. C. A. L. Reed presented the following resolutions, which were adopted :

Resolved, That the published program be and is hereby adopted as the order of exercises for the present meeting.

Resolved, That all papers passed when called shall not be again called until the conclusion of the next succeeding session.

Also, the following amendment to the By-laws:

"That Article 5 of the By-laws be and is hereby amended by striking out the word 'thirty' and inserting therefor the word 'twenty;' this amendment to go into effect after the present meeting."

This amendment was adopted without dissent.

On motion of Dr. X. C. Scott the recommendation from the American Medical Association was adopted, and the communication from the West Virginia Medical Society was referred to a committee of four, to be appointed by the President, to confer with the committee in this State appointed by the American Medical Association on that subject.

The Secretary's report was referred to the Committee on Finance.

The Committee on Finance was filled by the appointment of Drs. B. B. Leonard, E. Stanley, N. Gay and T. V. Fitzpatrick, *vice* Drs. E. Sinnett, W. S. Hough, D. P. Allen, *absent*, and E. W. Howard, *deceased*.

The Committee on Admissions was filled by the appointment of Drs. E. I. Ricketts, J. F. Baldwin, X. C. Scott, and W. Graefe, *vice* Drs. C. F. Clark, C. P. Landon, H. M. Brown and A. W. Ridenour, *absent*.

The Committee on Publication presented the following report, which was referred to the Committee on Finance:

The Committee met at Columbus, June 6th, 1890, and appointed the undersigned a committee to edit and publish the "Transactions."

Proposals to print the "Transactions" were submitted to two publishing houses in Toledo and one in Cleveland.

Bids were based on an edition of 600 copies, 300 pages, 37 lines to a page, printed on 50 lb. paper, bound as per sample.

The following bids were received:

The B. F. Wade Co., Toledo	$364 80
The Blade Printing & Paper Co., Toledo	340 00
The Williams Publishing Co., Cleveland	340 00

The only advantage of the Blade Printing & Paper Co.'s bid was in the cost of pages in excess of 300, which was 88 cents a page as against $1.00 a page by the Williams Co.

The bid of the Blade Co. was therefore accepted, and the work has been done in a satisfactory manner.

Very respectfully,

G. A. COLLAMORE,

Editor.

By request of the chairman, the Special Committee on "Bill for the Protection of Physicians," etc., was continued one year.

Dr. L. B. Tuckerman, on leave, presented the following, which was referred to the Committee on Legislation :

An Act to provide for the Protection of the Public against the Practice of Medicine and Surgery by Incompetent Persons.

This act shall provide:

1st. For a State Board of Medical Examiners appointed by the Governor, such Board to consist of —— persons of good repute for learning and skill in anatomy, physiology, chemistry, histology, pathology, diagnosis of disease, materia medica and the physiological action of remedies, surgery, obsterics, preventive medicine and medical jurisprudence.

2d. For the thorough examination in the above branches of all who hereafter may desire to undertake the practice of medicine or surgery within the State, and the issue of licenses to such as shall establish their fitness to be entrusted with the responsibilities devolving upon a practitioner of medicine and surgery.

3d. For the registration of and the issue of licenses to all such as are now practicing medicine and surgery within the State in conformity with the laws thereof now in force.

4th. For the efficient prosecution of unlicensed practitioners.

The President announced the following Committee on Nominations: Drs. Geo. Goodhue, P. Beeman, W. J. Scott, C. A. L. Reed and P. P. Pomerene.

A paper was read by Dr. R. Harvey Reed, on "The Surgical Treatment of Chronic Catarrhal Appendicitis."

Discussed by Drs. M. Stamm, of Fremont, C. A. L. Reed, E. I. Ricketts and R. B. Hall, of Cincinnati, W. J. Scott, of Cleveland, and D. N. Kinsman, J. F. Baldwin and T. W. Jones, of Columbus.

A paper was read by Dr. W. T. Corlett on "Notes on the Treatment of Syphilis ; its Evolution and Present Status."

Discussed by Drs. D. N. Kinsman, of Columbus, and S. S. Thorn, of Toledo.

A paper was read by Dr. R. B. Hall on "What Cases shall be Drained after Abdominal Section?"

Discussed by Drs. E. I. Ricketts, R. H. Reed, of Mansfield, and S. S. Thorn.

Society adjourned to 8:15 P. M.

EVENING SESSION.

The Society met at 8:15 P. M., President Conklin in the chair.

The minutes of the preceding session were read and approved.

A paper was read by Dr. H. J. Herrick on "Carcinoma, a Form of Perverted Nutrition."

Discussed by Drs. D. N. Kinsman, C. E. Beardsley, of Ottawa, and M. Stamm.

A paper was read by Dr. S. L. McCurdy on "Spinal Supports."

A paper was read by Dr. C. S. Hamilton on "Compound Ganglia: Treatment by Operation."

A paper was read by Dr. F. C. Larimore on "Three Cases Radical Cure of Hernia by the use of the Buried Antiseptic Animal Suture."

Discussed by Drs. R. H. Reed and W. E. Wirt, of Cleveland.

Society adjourned to 8:30 A. M., June 18th.

SECOND DAY—MORNING SESSION.

The Society convened at 8:30 A. M., President Conklin in the chair.

The minutes of the preceding session were read and approved.

The Treasurer and Librarian presented the following report, which was referred to the Committee on Finance:

As Treasurer and Librarian I respectfully report as follows:

My last report at Columbus, June 4th, 1890, showed a balance due Society or on hand	$ 24 71
Initiation fees and dues since date of last report	898 00
Total	$922 71
Since above report and date the following bills (per vouchers) for use of Society have been paid:	
Gazette Printing House	$ 4 75
W. F. Brokaw, Stenographer	75 00
Eddy & Johnston "	32 00
Gazette Printing House	2 75
Postage Stamps	11 00
Express Charges, Trans.	32 60
Freight Charges and Drayage	3 38
Express Charges	80
Treasurer and Librarian	125 00
Postal Cards and Stamps	4 50
Blade Printing Company, Toledo	385 95
" " " "	4 76
G. A. Collamore, Sec., salary	125 00
" " " Editor Transactions	25 00
" " " Expenses	51 68
	$894 17
Cash on hand	28 54
Total	$922 71

As Librarian I report copies of Transactions, 1870-1-2-4-5-6-7-8, making some 60 copies; 1881 and 1882, about 100; 1887, 50 copies; 1888, 75 copies; 1889, 150 copies; 1890, 125 copies. Board of Health Reports from the different States for several years past in exchange. A few copies of Consti-

tution and By-laws of this society in pamphlet form. Our exchanges for the past year were as follows:

New York Medical Association, 1890, 1891; New York Medical Society, 1890; North Carolina Medical Society, 1890; Delaware Medical Society, 1889; Wisconsin Medical Society, 1890; Connecticut Medical Society, 1890; Michigan Medical Society, 1890; West Virginia Medical Society, 1890, 1889; Illinois Medical Society, 1889, 1890; New Jersey Medical Society, 1890; Massachusetts Medical Society, 1890; Florida Medical Society, 1890; Pennsylvania Medical Society, 1889, 1890; Texas Medical Society, 1890; Maine Medical Society, 1890; Tennessee Medical Society, 1890; Alabama Medical Society, 1890; Iowa Medical Society, 1890; Vermont Medical Society, 1890; Georgia Medical Society, 1890; New Hampshire Medical Society, 1890; Maryland Medical Society, 1890; Nebraska Medical Society, 1890; Colorado Medical Society, 1890; Oregon Medical Society, 1889; Mississippi Medical Society, 1890. State Board of Health, Pennsylvania, 1888; Michigan, 1890. Sanitary Convention, Michigan, 1890. Catalogue Massachusetts Medical Society, 1890. Report on Medical Education, Illinois State Board of Health, 1890.

Respectfully,

T. W. JONES,

Treas. and Lib.

A paper was read by Dr. J. C. Reeve on "A. C. E. Mixture." Discussed by Drs. C. A. L. Reed, J. McCurdy, of Youngstown, R. H. Reed, H. J. Herrick, X. C. Scott, of Cleveland, F. C. Larimore, of Mt. Vernon, J. N. Stockstill, of New Carlisle, D. H. Brinkerhoff, of Fremont, N. Gay and J. F. Baldwin, of Columbus, S. S. Thorn, of Toledo, T. A. Reamy, of Cincinnati.

A paper was read by Dr. D. N. Kinsman, on "Influenza." Discussed by Drs. G. Goodhue, of Dayton, E. Sinnett, of Granville, N. Gay, W. B. Davis, H. J. Herrick, A. B. Richardson, of Cincinnati.

A paper was read by Dr. C. W. Tangeman on "Convergent Squint and its Cure."

A paper was read by Dr. A. R. Baker on "Some Facts Every Practitioner ought to know about Squint."

The two preceding papers were discussed by Drs. S. L. McCurdy, of Dennison, and X. C. Scott.

Adjourned to 2 P. M.

AFTERNOON SESSION.

The Society met at 2 P. M., President Conklin in the chair.

The minutes of the preceding session were read and approved.

The Committee on Finance, by W. T. Corlett, Chairman, reported that the accounts of the Treasurer and Secretary had been found correct, and recommended that the annual dues be fixed at *two dollars.*

Report adopted.

The Special Committee on "Organization of County Societies," etc., submitted the following report by Dr. A. R. Baker:

The committee at the last meeting recommended the following amendment to the constitution:

Article III., Section 10. All members in good standing in county medical societies shall be permanent members of this society, and shall be entitled to all its rights and privileges, including a copy of the annual transactions, excepting that the business of the sessions shall be transacted by delegate members.

After discussion by Drs. X. C. Scott, J. F. Baldwin, D. J. Merriman, of Painesville, J. McCurdy, T. A. Reamy, the following amendment, offered by Dr. R. Harvey Reed, was adopted: "*provided*, that all members of the Society up to the present date shall be entitled to vote." Dr. X. C. Scott moved to amend so as to include *district* as well as *county* medical societies. After discussion by Drs. S. B. Hiner, R.

H. Reed and T. A. Reamy, this amendment was also adopted.

Article III., Section 10, of the Constitution, will therefore read as follows: "All members in good standing in county and district medical societies shall be permanent members of this society, and shall be entitled to all its rights and privileges, including a copy of the annual 'Transactions'; excepting that the business of the session shall be transacted by delegate members: *provided*, that all members of the Society up to the present date shall be entitled to vote."

On motion of Dr. R. Harvey Reed, the Secretary was instructed to send a telegraphic message of sympathy to the wife of Dr. A. W. Ridenour, now very sick.

The Society now proceeded to the election of officers, which resulted as follows:

Dr. G. A. Collamore, of Toledo, was elected President.

The Committee on Nominations presented the following report, which was adopted, and the Secretary was ordered to cast the ballot of the Society for the following:

1st Vice-President—Dr. X. C. Scott, Cleveland.
2d Vice-President—Dr. A. J. Gawne, Sandusky.
3d Vice-President—Dr. A. R. Baker, Cleveland.
4th Vice-President—Dr. F. D. Bain, Kenton.
Secretary—Dr. T. V. Fitzpatrick, Cincinnati.
Assistant Secretary—Dr. Auguste Rhu, Marion.
Treasurer and Librarian—Dr. T. W. Jones, Columbus.
Member of Committee on Finance for five years—Dr. H. J. Herrick, Cleveland.
Member of Committee on Legislation for five years—Dr. D. J. Snyder, Columbus.
Member of Committee on Ethics for five years—Dr. S. S. Thorn, Toledo.
Member of Committee on Publication for five years—Dr. E. S. McKee, Cincinnati.
Member of Committee on Admissions and Medical Societies for five years—Dr. A. F. House, Cleveland.

The place selected for the meeting in 1892 was Cincinnati, and the time appointed was the first Wednesday in May.

Dr. T. A. Reamy made a "Report of Cases, with Comments."

Dr. D. R. Silver read a paper on "A Plea for a More Extended Supervision of the Parturient Woman."

Dr. D. Tod Gilliam read a paper on "Operative Treatment of Uterine Cancer."

A paper was read by Dr. J. F. Baldwin on "A Rare Case of Pelvic Dropsy; Operation; Cure."

A paper was read by Dr. M. Stamm on "The Value of Draining the Pelvis in Case of Bleeding after Operation."

Dr. Reamy's paper was discussed by Drs. E. I. Ricketts, of Cincinnati, and H. J. Herrick.

A paper was read by Dr. A. B. Walker on "Salpingitis, with Report of two Cases."

Dr. Gilliam's paper was discussed by Drs. W. H. Humiston and A. F. House, of Cleveland, and T. A. Reamy.

Dr. Baldwin's paper was discussed by Dr. T. A. Reamy.

Dr. Walker's paper was discussed by Dr. W. H. Humiston.

The President appointed as Committee on the communication from the West Virginia Medical Society the following: Drs. D. P. Allen, J. C. Reeve, J. F. Baldwin, R. H. Reed.

Society adjourned to 9 P. M.

EVENING SESSION.

The Society met at 9 P. M. at the Club House at Cedar Point, Vice-President Snyder in the chair.

The President, Dr. W. J. Conklin, delivered the annual address, the subject being "A Page of Medical History: Molière and the Doctors."

Society adjourned to 9 A. M. June 19.

THIRD DAY—MORNING SESSION.

The Society met at 9 A. M., President Conklin in the chair.

The minutes of the two preceding sessions were read and approved.

Dr. T. D. Strong, delegate from New York State Medical Association, and Dr. D. D. Loop, delegate from the Pennsylvania State Medical Society, were introduced by the President and invited to a seat on the platform.

On motion of Dr. E. I. Ricketts it was voted that the thanks of the Society are due and are hereby presented to the medical profession of Sandusky, for the excellent entertainments extended to the Society.

Dr. C. A. L. Reed rose to a question of privilege.

"I beg leave to speak to a question of privilege. It should be remembered that last year at Columbus, my distinguished friend Dr. Reamy and myself exchanged some views on the treatment of cancer of the uterus, and that the discussion was reported at length in the Transactions. By an error for which I presume I am responsible, as the proof slips were submitted to me, the part of the criticism which I intended for one speaker was applied to Dr. Reamy. I allude to this portion.

"'But this is not all of the pathological wonders to which my distinguished friend has treated us. He has given us his theory of cancer, but I am forced to admit that I cannot translate his words into things. I gather, however, that from a study of 'certain dyscrasias,' and 'nascent organized materials,' and 'abnormal forms of growth,' and 'weakened conditions of the system,' and 'leucocytes' and things, Dr. Reamy concludes that clinical experience and common sense would seem to establish the conclusion that cancer is a constitutional and not a local disease! Then why, in the name of consistency, does he operate upon any of these cases at all? If the disease is constitutional, any effort at its local eradication is mere madness; yet this distinguished surgeon stands before us to-day as an advocate, under certain conditions, of both high amputation and vaginal hysterectomy. My regard for him prompts me to indulge the hope that he will extricate himself from his present predicament and effect some sort of reconciliation between his philosophy and his practice.'

"The criticism, which I made in perfect good faith, was intended to apply to another speaker, whom I would name if

he was here. It was simply an error, for which I desire to make amends."

A paper was read by Dr. T. V. Fitzpatrick on "Tonsillotomy and After-Treatment."

Discussed by Drs. A. DeVilbiss, T. Hubbard, J. North and S. S. Thorn of Toledo, J. McCurdy, A. W. Thrasher, of Cincinnati, A. B. Walker, of Canton, and B. O. Williams, of Martin's Ferry.

A paper was read by Dr. Geo. Goodhue, on "Intubation.

Discussed by Drs. T. Hubbard and J. North.

A paper was read by Dr. A. B. Thrasher, on "Papilloma of Larynx."

A paper was read by Dr. W. B. Davis on "Tests for Albumen."

Discussedby Drs. J. B. Hough, of Waynesville, C. R. Merriman, of Painesville, J. North and Dixon.

Dr. J. McCurdy offered the following resolutions, which were adopted:

WHEREAS, All our charitable institutions are now under political influence, the baneful effects of which are painfully evident by the removal of their officers as one or other of the parties succeeds; and

WHEREAS, These frequent changes work great hardships to both inmates and institutions in many instances by the removal of officers who are peculiarly well qualified by natural abilities and special training for their work; and

WHEREAS, The great body of the inmates of these institutions are keenly alive to their actual conditions, and feel their great loss by the placing of inexperienced, frequently incompetent, and often cruel, officers over them, and whereas their condition is that of absolute helplessness; therefore

Resolved, That as members of the medical profession of the State of Ohio, we earnestly unite in asking our Governor and all the members of both branches of our Legislature to enact such laws as will subject all officers and employes to special examinations for their respective positions, and when

proven to be qualified will be placed under civil service rules, and will not be removed except for cause.

Resolved, That in making this request we do not arraign one political party for its bad faith, but earnestly ask all members of all parties to join in putting these great charities upon a non-partisan basis.

Resolved, That what we now ask is not an untried experiment, but one fully established in several States, especially in the great State of New York, where it is at this moment working with gratifying success.

Resolved, That we ask our State government to establish a board of examiners for ascertaining the qualifications of all applicants for these institutions.

Resolved, That we ask not only all members of the medical profession, but all good citizens interested in civil service reform, to join us in making a personal request that this necessary legislation be enacted at the earliest possible time.

A paper was read by Dr. A. B. Richardson on "Home *versus* Hospital Treatment of the Insane."

A paper was read by Dr. W. A. Dickey on "Hyperemesis Gravidarum."

Dr. T. Hubbard offered the following resolution, which was adopted:

The Ohio Medical Society, in convention assembled, call the attention of the State Board of Health to the necessity for the adoption and enforcement of a sanitary regulation which shall require that all cases of pseudo-membranous laryngitis, whether of the diphtheritic or so-called croupous type, be reported to the local health department the same as other contagious diseases.

The Secretary is hereby directed to forward the same to the Secretary of the State Board of Health.

Dr. C. A. L. Reed moved that the Secretary be instructed to notify the Ohio State Pharmaceutical Association that

the State Medical Society appreciates the communication lately received from that association. Carried.

Society adjourned to 3 P. M.

THIRD DAY—AFTERNOON SESSION.

The Society met in the cabin of the Steamer "City of Sandusky" *en route* to Put-in-Bay, President Conklin presiding.

Dr. J. B. Hough, of Waynesville, on leave, presented the following resolutions, which were unanimously adopted:

WHEREAS, There exists in the regular medical profession an earnest and widespread desire to raise the standard of medical education, by requiring of its under-graduates more years of study and a better chemical training; and

WHEREAS, The increased and increasing number of medical schools, especially those located in small and out-of-the-way places, where clinical or hospital advantages cannot exist, is plainly inconsistent with the foregoing preamble: therefore, be it

Resolved, By The Ohio State Medical Society, that the formation of new medical schools at small towns or villages remote from hospitals, clinical advantages and lines of travel, should be and hereby *is strongly condemned*. Also,

Resolved, That regularly educated physicians, in good professional standing, who encourage in any way, either directly or indirectly, the establishment of medical schools, such as above condemned, should be advised and hereby *are* advised against such course as being detrimental to professional interest and honor.

Also the following resolution, which was referred to the Committee on Ethics:

Resolved, that the medical school quite recently opened as a department of the so called "National Normal University" at Lebanon, Ohio, is one of the kind above referred to, and one that should not reasonably expect to receive the sanction and support of the regular medical profession.

The Secretary was instructed to notify the Lebanon Medical Society of this action.

The following is the consolidated report of the Committee on Admissions and Medical Societies, and the following were duly elected members of the Society:

NAME.	P. O. ADDRESS.	WHERE GRADUATED.
Baker, J. P.	Findlay,	Starling Med. College.
Barnes, W. T.	Fredericksburg,	Charity H'sp. M'd Col'ge.
Benner, C. T.	Tiffin,	Bellevue H'sp. M'd. Col'ge.
Bennett, W. C.	Iberia,	M'd. Dept. Wooster Univ.
Bunce, W. C.	Oberlin,	M'd. Dept. Wooster Univ.
Calvin, J. H.	Huron,	M'd. Dept. Wooster Univ.
Campbell, John	Galion,	Cleveland Med. College.
Ervin, N. B.	Gibsonburg,	M'd. Dept. Wooster Univ.
Foster, C. A.	Washington C. H.	Col. Phys. and Surg. N. Y.
Gibbon, H. B.	Tiffin,	Cincinnati Med. College.
Graefe, Charles	Sandusky,	M'd. D'pt. Wooster Univ.
Hamilton, C. S.	Columbus,	Columbus Med. College.
Hasencamp, O.	Toledo,	Med. D'pt. Univ. Tenn.
Hobson, J. A.	Flushing,	Miami Med. College.
James, A. D.	Mt. Gilead,	Miami Med. College.
Keller, B. T.	Bloomingburg,	Univ. Maryland.
Leahy, Maurice	Tiffin,	Bellevue H'sp. M'd. Col'ge. / M'd. D'pt. Univ. Wooster.
Lefevre, Edwin.	Sidney,	Med. Dept. Howard Univ.
Leonard W. W.	Delaware,	Miami Med. College.
Love, M. J.	Bloomingville,	L. I. H'sp. Med. College.
McGlenn J. A.	Mt. Pleasant,	Jefferson Med. College.
Marquart, O. M.	Osborn,	Miami Med. College.
Merz, Charles H.	Sandusky,	Med. Dept. Wooster Univ.
North, John	Toledo,	University of Michigan.
Peck, G. S.	Youngstown,	Med. Dept. Wooster Univ.
Peter, Robert	Canal Dover,	Jefferson Med. College.
Richardson, A. B.	Cincinnati,	Bellevue H'sp. M'd. Col'ge.
Shallioll, R.	Winesburg,	Ohio Med. College.
Stamm, M.	Fremont,	Univ. Berne, Switzerland.
Tuckerman, L. B.	Cleveland,	L. I. H'sp. Med. College.

Von Klein, C. H.	Cleveland,	Univ. Prussia.
Ward, C. L.	Cridersville,	Ky. School of Med.
Williams, B. O.	Martin's Ferry,	Med. Dep't. Univ. Pa.
Williamson, M. S.	Findlay,	Med. Dep't. Univ. Wooster
Wirt, W. E.	Cleveland,	Col. Phys. and Surg. N. Y.
Wright T. M.	Troy,	Ky. School of Med.
Zopfi, W. J.	Findlay,	Univ. Buffalo.

The following Medical Societies were made auxiliary to the State Medical Society:

Medical Association of Delaware Co.

Fayette Co. Medical Society.

Hancock Co. Medical Society.

The following Auxiliary Societies were represented by delegates, as follows:

Academy of Med. of Cincinnati—A. B. Richardson.

Ashtabula Co. Med. Society—E. D. Merriam.

Cuyahoga Co. Med. Society—L. B. Tuckerman.

Erie Co. Med. Society—Charles Graefe, E. Stanley.

Delaware Co. Med. Association—W. N. Leonard, J. L. Williams.

Fayette Co. Med. Society—B. F. Keller.

Sandusky Co. Med. Society—D. H. Brinkerhoff, M. Stamm.

Toledo Med. Association—O. Hasencamp.

Wayne Co. Med. Society—Chas. A. Lerch.

There being no further business the Society adjourned to meet in Cincinnati on the third day of May, 1892, at 2 P. M.

W. J. Conklin, *President.*

G. A. Collamore, *Secretary.*

LIST OF AUXILIARY SOCIETIES.

WITH DATE OF ADMISSION.

Academy of Medicine of Cincinnati 1875
Adams County Medical Society 1888
Allen County Medical Society 1867–1887
Ashland County Medical Society 1865
Ashtabula County Medical Society 1878
Bay City Medical Society 1880
Belmont County Medical Society 1887
Brown County Academy of Medicine 1877
Butler County Medical Society 1852
Central Ohio Medical Society 1869
Champaign County Medical Society 1853–1879
Cincinnati Medical Society 1875
Clarke County Medical Society 1848–1852–1877
Clermont Medical Association 1854
Clinton County Medical Society 1879
Columbus Medical Club 1885
Cuyahoga County Medical Society 1879
Erie County Medical Society 1850–1877
Fayette County Medical Society 1891
Geauga County Medical Society 1882
Green County Medical Society 1888
Hancock County Medical Society 1891
Hempstead Academy of Medicine 1885
Highland County Medical Society 1877
Hildreth District Medical Association 1886
Holmes County Medical Society 1869
Jefferson County Medical Society 1859
Lebanon Medical Society 1879
Licking County Medical Society 1877

Mahoning County Medical Society 1879
Marion County Medical Society 1879
Medical Association of Delaware County 1891
Meigs County Medical Society 1850
Miami County Medico-Chirurgical Society 1851
Miami Valley Medical Society 1879
Montgomery County Medical Society 1851
North Central Ohio Medical Society 1881
Northeastern Ohio Medical Association 1870
Northwestern Ohio Medical Association 1869
Piqua Society of Clinical Medicine 1886
Portage County Medical Society 1869
Preble County Medical Society 1849
Ross County Medical Society 1876
Sandusky County Medical Society 1880
Shelby County Medical Society 1852
Southeastern Ohio Medical Association 1890
Stillwater Medical Society 1873
Summit County Medical and Surgical Society 1886
Toledo Medical Association 1857
Trumbull County Medical Society 1868
Tuscarawas County Medical Society 1851
Union Medical Association of Columbiana and adjoining Counties 1879
Washington County Medical Society 1886
Wayne County Medical Association 1850
Wood County Medical Society 1877

WILLIAM JUDKINS CONKLIN.

William Judkins Conklin, A. M., M. D., was born in Sidney, O., December 1, 1844. His literary education was obtained from the public schools of his native town, and from the Ohio Wesleyan University, Delaware, O., from which he was graduated Bachelor of Arts in 1866. Beginning the study of medicine under his father, Dr. H. S. Conklin, a prominent physician of the Miami Valley and President of the Ohio State Medical Society in 1861, he received his medical degree from the Ohio Medical College in March, 1868.

In the spring of 1869 he was appointed Assistant Physician to the Dayton Hospital for the Insane, which position he resigned in December, 1871, to engage in general practice in Dayton. He was subsequently appointed by Gov. Hayes a member of the Board of Trustees for the above institution, and served as local Trustee and Secretary for two years.

He was Secretary of the Ohio State Medical Society in 1872 and 1873. From 1875 to 1886 he was a member of the Faculty of Starling Medical College, first as Professor of Physiology, and afterwards as Professor of Diseases of Children. He served several terms as a member of the Dayton School Board, and for the past ten years has been connected with the management of the Dayton Public Library. He has been a member of the surgical staff of St. Elizabeth Hospital since its organization in 1878. He joined the Ohio State Medical Society in 1868.

A PAGE OF MEDICAL HISTORY: MOLIÈRE AND THE DOCTORS.

W. J. CONKLIN, M A., M. D.

"Your Molière with his comedies is a fine, impertinent fellow. It is like his impudence to put upon the stage such worthy persons as the physicians."—*Le Malade Imaginaire.*

Medical men and their doctrines have always been inviting targets for wits and satirists. The most famous satirists of the medical profession before Molière, were Petrarch and Montaigne. The latter devotes one of the longest chapters in his work to a bitter tirade against medicine, which he enforces with all the orginality of his wit and with his usual wealth of illustration. While the influence of Montaigne's essay is plainly traceable in the writings of Molière, it still continues to furnish inspiration to a host of weaker but equally hostile critics—poking fun at doctors can not yet be classed among the lost arts. But Molière, whom Hazlitt calls one of the greatest comic geniuses that ever lived, is the acknowledged master of medical satire. He was especially fitted by education and genius to appreciate the ludicrous side of seventeenth century medicine. The stormy struggle which attended the birth of the modern sciences in France, and which drew into its vortex all classes of society, popes, kings and laity as well as doctors, challenged his keenest wit and sharpest sarcasm. It is generally conceded that his comedies played no unimportant part in shaping the medical history of this unique period.

Jean Baptiste Poquelin was born in Paris in 1622. His father was an upholsterer, holding an appointment

in the royal household, and of some wealth and social position. He received a liberal education at the famous Jesuit College de Clermont, then the most popular school in France, and which, under the name of College Louis Le Grand, still continues its educational work. Here he had for instructor in philosophy the learned Gassendi, and for fellow-students the Prince de Conti, the poet Hessnaupt, the rollicking Chapelle, the traveler-physician Bernier, the intimate friend of his later years, and the famous Cyrano de Bergerac, who killed ten men in duels to maintain the honor of his own enormous nose.

After the completion of his college course, young Poquelin, following the usual custom, went to Orleans and enrolled himself as a student of civil law. He, however, soon abandoned the law for a more congenial profession, and under the assumed name of Molière began that career which has made his name immortal.

The first twelve years of his stage life, passed mostly in the provinces, the head of a successful troupe of vagabond players, presents nothing of especial interest. On its return to Paris in 1658, the Illustre Théâtre, Molière's company, was invited through the influence of friends to play before the king. This was the long sought opportunity, and grandly did Molière's magnificent genius respond to it. Never did a strolling player appear before a more brilliant audience. Assembled in the old guard-room of the Louvre to welcome the coming of him who was soon to be recognized as the greatest of modern comic dramatists, was Anne of Austria, the queen-mother, still imposing and splendid in the supreme authority she was soon to lose; Mazarin, the cunning cardinal, plotting some deed of statecraft while he seemed to listen; Louis, the Grand, in the vigor of his young manhood, the pride and hope of France, and Philip, his brother, the "curled darling," who, dressed in female robes, set the fashions of the day. Around this central group were gathered obsequious courtiers, ruffled dames, veteran war-

riors, *precieuses* from the *salon* of Madame de Rambouillet, that famous center of super-refinement, players from the Hotel de Burgoyne, jealous of their new rivals, and all the nobility which fluttered about a court the most splendid and dazzling in history. Though debarred by strict etiquette, the doctors of the court were doubtless present and joined in the general laugh, little thinking that they themselves would soon be the butt of the dramatist's ridicule.

From this moment Louis became Molière's friend and protector. The sincerity of his friendship is shown by many acts of kindness. He granted pensions, shielded him from the attacks of the nobles and clergy, and, to hush the vilest of slanders, stood god-father to his child. Molière amply repaid his patron by writing that series of brilliant comedies which not only splendidly set off the royal *fêtes* at Versailles and Fontainebleau but which mark an era in the history of dramatic literature.

Molière's earlier pieces, modeled after the Spanish and Italian schools, gave but little evidence of the master's power. Not until later in life, when he found his real field in the shams and follies of his day, did he paint the Tartuffes, Sganarelles, Argans, Dandins, Orgons, those eternal types of human character which are independent of time or country and are as true to-day as the hour in which they were conceived.

Nearly all commentators lay stress upon the implacable hatred which Molière bore to doctors and their calling. A careful study of his writings proves that the indictment has been overdrawn. Molière had two objects in view: the first and paramount one was to please the king and make the people laugh. To fail in this, meant another period of vagabondism in the provinces. Nothing was too humble or too sacred for his purpose. The doctors were fair game and easy to ridicule. Everybody, when well, laughs at doctors, and no one, not even the doctors, is seriously hurt by it.

His second object was to chastise the follies, foibles and extravagances of his age. An intense hater of shams, cant, hypocrisy, pedantry and all that goes to make up the *Tartuffism* of theology, science or society, he attacked them with an earnestness and boldness rarely seen in the pulpit and never on the stage. He, however, bore no rancor, and neither fawned nor flattered in an age of flattery. The nobility, the *bourgeois*, the clergy, doctors, *precieuses*, all classes of society, all stations in life, if vulnerable, felt the sting of his sarcasm.

With few exceptions, the shafts of his humor, like the arrows of Tell, pierced the very center of the foibles at which they were aimed, without wounding head or heart.

At times, however, he offended good taste, and abused the license of the stage by his glaring personalities or by parading before the foot-lights the thinly disguised counterparts of living men, but the physicians fared no worse in this respect than did the members of other professions. This recklessness, only indulged in because the strong arm of the king was behind him, often brought him into trouble, and there is a well authenticated story of an angry marquis who severely punished him for his stage insolence.

It is a significant fact that Molière's dislike of doctors became most apparent in the later years of his life, when the shadows were rapidly gathering around him, and is, probably, largely the expression of his own embittered disposition.

Carping critics pursued him and resorted to the coarsest and most cruel insinuations against himself and wife. The indiscretions and heartless coquetry of his young and giddy wife, of whom he was passionately fond, led to quarrels and separation. For years, though living under the same roof, they only met on the stage. To this list of woes we must add ill-health. Molière had a serious illness in 1666, while *L'Amour Médecin* was running, and from this time until his death, six years later, his

health was precarious. A harassing cough, accompanied with occasional hemorrhages, due either to thoracic aneurism or to phthisis pulmonalis, interfered with his work, and forced an abstemious life, little in accord with his tastes or luxurious surroundings. "How much a man suffers ere he dies" was his sad exclamation before going to the theatre on the evening of his death. Under these circumstances, it is not surprising that one so sensitive and ambitious should at times break forth in bitter invective against the art which could ameliorate but not cure. "You have a doctor," said the king to him when walking together in the royal garden, "what does he do for you?" "Sire," replied the wit, "we walk together, he prescribes remedies which I do not take, and I get well."

But few phases of medical life escaped Molière's keen observation. The range of his information on medical subjects is phenomenal. It is certainly the prerogative of the highest genius to grasp, without special training, the salient points of a science or art so complicated as medicine.

So long as he caricatures the ever-present charlatan or medical buffoon, stock characters of every playwright; or paints those comical portraits of the medical *Tartuffes*, with their big wigs, bad Latin and pompous manners, we can join in the general laugh. We can equally enjoy the sharp shafts of satire with which he impales those purists who so stubbornly fought the discoveries which heralded the birth of modern medicine; or the quaint humor with which he exposes that peculiar conservatism of the seventeenth century, which, like the stern lights of a ship, only illumined the track passed over. But the dramatist did not stop here: he sought bigger game. In *L'Amour Médecin* and *Le Malade Imaginaire*, the best of his so called medical comedies, he not only made sport of the most prominent doctors of his period, but fiercely assailed the very philosophy of medicine.

Molière, however, did not carry his enmity to the profession into his personal friendships. With three physicians, at least, he was on intimate terms: Bernier, a fellow-student at de Clermont; Lienard, an extravagant Cartesian, who sought to reconstruct the whole system of medicine in accordance with the physical principles of Descartes, and with Mauvillain, his physician.

Mauvillain has passed into history as Molière's medical mentor, and it is not improbable that he used the dramatist's wit to even up old scores, of which he is said to have had many, with his colleagues. Mauvillain was the son of a surgeon of local fame and also librarian to Richelieu. The Cardinal out of friendship to the father became sponsor for the son, gave him his own surname d'Armand and looked after his education. Mauvillain was a man of the world, agreeable in his manners, of good professional attainments, but hot-headed and impulsive. He was twice expelled from the Faculty of Medicine for irregularities of conduct; once for giving with Guénaut, Béda and other antimonialists a certificate recommending Orvietan, a famous nostrum which figures prominently in "*The Imaginary Invalid*," and to which Scott devotes an interesting chapter in *Kenilworth*. At another time, in an altercation with Blondel, the dean of the Faculty, over some matter of professional etiquette, he created a sensation by snatching the dean's bonnet from his head and carrying it in triumph from the hall. Expulsion duly followed. Mauvillain was a staunch supporter of chemical remedies, a recognized leader in the antimonial war, and at its close was elected dean of the Faculty. The only office which Molière is known to have ever asked of the king was the canonry of Vincennes for Mauvillain's son. The humorous letter in which the request is made shows that he fully recognized the strained relations which existed between himself and the doctors.*

* "Sire—A very honest doctor, whose patient I have the honor to be, promises and wishes to undertake before a notary that he will

To correctly interpret the medical situations in Molière's plays, it will be necessary to glance hastily at the condition of France and medicine at the period in which he wrote.

This period was one of great glory to France. Under the crafty statesmanship of the Cardinals, Richelieu and Mazarin, her political power at home and abroad was greater than at any time in her history, excepting alone the reign of Napoleon. Louis XIV., whom Bolingbroke calls "the greatest actor of majesty that ever filled a throne," so impressed his personality upon the world that an epoch in the progress of civilization has been called by his name. He aspired to the title of "Protector of Letters," and by nurturing all departments of learning, he combined, says Döllinger, the character of a Mæcenas with that of a great ruler. The medical profession received full recognition. To physicians of merit he granted titles of nobility and coats of arms, and exempted them from certain contributions to the crown. "We cannot justly withhold," says the king, "such marks of honor to men of learning, who by their devotion to a noble profession are entitled to a rank of high distinction." With the reign of Louis XIV. are associated many of the most eminent men that France has produced. Corneille, Racine, Molière and La Fontaine spoke into life the modern drama; Pascal, Bourdaloue and the golden-mouthed Bossuet added the "sparkle of literature and oratory to the dignity of religion"; Descartes, Malebranche, and Gassendi advanced the limits of scientific knowledge; Poussin

make me live thirty years longer, if I can procure a favor from your Majesty. As to his promise I told him I did not ask for so much, and should be satisfied if he would undertake not to kill me. The favor, Sire, is the canonry of your royal chapel of Vincennes, now vacant. Dare I ask this favor of your Majesty, the very day of the resurrection of 'Tartuffe,' revived by your goodness? By the first favor I am reconciled to the devotees, and by the second I should make my peace with the doctors. For me these are doubtless too many favors at once, but perhaps not too many for your Majesty, and I await with respectful hope the reply to my petition."

and Claude Lorrain created the modern school of art, and LaBruyére, Fénelon, Rochefoucauld and Madame de Sevigne contributed to the literary splendor of this golden age.

The medical history of this period is full of lights and shadows. The seventeenth century is the dividing line between ancient and modern medicine, and has the turbulent history which always belongs to transitional periods.

The revival of Greek learning in the preceding century gave to medicine the same forward impulse that it did to all other branches of learning. The first and most noticeable effect was to dispel the false philosophy of the Arabs, which settled like a pall over the medical world with the downfall of the school of Alexandria, and to substitute for it the doctrines of Hippocrates and Galen, thus leading medicine back to its fountain head. Taking the best possible view, medicine at the beginning of this "Reform Period," as Renouard aptly styles it, was in a deplorable condition; it was a mixture of dogmatism, empiricism and mysticism. To question the authority of Hippocrates or Galen was heresy, and skepticism was nearly as rare in medicine as in religion. The spirit of the times is happily shown in the following extract from the statutes of the academy at Helmstädt:

> We desire the medical art, even as it was rightly and wholly fixed and handed down, under the guidance of God, by the inspired artists Hippocrates, Galen and Avicenna, to be preserved and diffused by teaching. We recommend that all Empirics and the 'Tetralogies' of Paracelsus, with other corruptions of medicine not agreeable to the doctrines of Galen and Avicenna, be banished entirely from the Academy.

Sects like the Rosicrucians and Electro-Conciliators, though possessed of considerable knowledge, cultivated the occult sciences, believed in the transmutation of metals, in the power of witchcraft, and pretended to cure disease by preternatural influences. Chemistry, still under the influence of her patron, Paracelsus, was strongly

tinctured with the mysticism of alchemy and astrology. Potable gold was held by many to be a sovereign remedy for every disease, and the sympathetic powder of Sir Kenelm Digby, Knight of Montpellier, threatened to supplant all other forms of wound treatment. A barbarous polypharmacy, the gift of the Arabs, prevailed, and medicines were administered in complex and incongruous mixtures, as if to make up by the number of ingredients for ignorance of the action of remedies.

At this juncture came Harvey, and with him came the possibility of modern scientific medicine. The knowledge of the circulation of the blood and the development of the higher animals from the egg, both of which we owe to the gifted Englishman, are acquisitions to general medicine, the grandeur and importance of which have not been surpassed by any discoveries since the dawn of civilization.

Other discoveries in medicine may have more directly benefited the race, as, for example, anæsthesia and vaccination, but there are none of which the immediate and remote consequences have been so marked. Now that spirit of inquiry and innovation which may be called the scientific movement was thoroughly inaugurated. Everywhere men whose knowledge and genius equaled their enthusiasm came to the front and assumed direction of the intellectual movement. In proof, we may cite Harvey, Silvius, Willis, Malpighi, Anselli, Pecquet, Peyer, Steno, Swammerdam, Mauriceau, DeGraaf, Sydenham, Boerhaave, and many others whose names and works are as familiar as household words, and whose contributions to medicine have never been excelled in scientific value.

"The acquisitions of this epoch," says Baas, "in the sphere of the facts of medicine were extraordinary, and the importance of the results of this century is, on the whole, equal to that of our own age, which has also so

greatly advanced this very side of medicine. In some respects it even surpassed the present century."

Not medicine alone but all of the collateral sciences felt the awakening influence. A Galileo carried the torch of regeneration into the dark domain of physics; a Kepler wrested from the planets the laws of their movements, and chemistry struggled to throw off the stigma of her alchemic origin.

These recent discoveries, however, necessitated a rearranging of medical doctrines, and brought about a strong tendency to the mechanical explanation of normal and abnormal processes, which was further encouraged by the growth of the mechanical philosophy of Descartes, and the contemporary progress of physics and the natural sciences. The time was ripe for the birth of systems of medicine, and the systems of Van Helmont, of the Iatro-Physical and Iatro-Chemical sects, came into existence.

We can only give here the briefest outline of the doctrines of these schools.

Van Helmont (1578–1644) came from a noble family in Brussels. After having received a liberal education, he applied himself to the study of medicine with the ardor of an enthusiast and the obstinacy of a fanatic. "He was the genuine son of his century, at once a mystic and realist." He attempted to remodel the pantheism of Paracelsus into a mystic system based upon chemical principles. The *archeus*, which played such an important rôle in the systems of the sixteenth century, reappears, with many complications and alterations; the symptoms of diseases were caused by the perturbations of this archeus, and medicines acted by modifying it. Although his doctrines simulated those of Paracelsus, they were freer from contradictions, and involved an actual though slight advance in scientific knowledge. He laid great stress upon the use of simple chemical remedies, abhorred blood-letting, and introduced newer

methods into the preparation of drugs. An eccentric genius, who vacillated from one calling to another, and who several times gave up his worldly possessions to lead a life of the strictest fanatical asceticism, he never obtained a large following, but several of the later sects borrowed liberally from his doctrines.

The *Iatro-Physical School* was an outgrowth of the new physiology. Its founder, Borelli of Naples (1608-1679), was at one time teacher of mathematics in Messina, but his quarrelsome disposition drove him from one university to another. He finally gravitated to Rome as one of that coterie of savants supported by Christina of Sweden, daughter of Gustavus Adolphus, after her conversion to Catholicism. For her he wrote his work, "*De Motu Animalium*," published after his death, which is regarded as marking an epoch in physiology. His latter days were passed in poverty and in begging through the streets of Rome. The dominant idea of this school was the dependence of the actions and functions of the body on physical and especially on mechanical principles. The movements of the bones and muscles were referred to the theory of levers; digestion was essentially a process of trituration, and nutrition and secretion were dependent upon the tension of the vessels. Mechanical theories were introduced into pathology in explanation of fever and other abnormal processes. The school, however, had but little influence on therapeutics.

The Iatro-Chemical School exerted a much greater influence over practical medicine than either of the above systems. Iatro chemistry was the successor of alchemy and the first near approach to the true science. The underlying principle of this school was not merely the use of chemical in addition to the traditional or Galenical remedies, but involved a theory of pathology radically different from the prevailing humoral pathology. It was a praiseworthy effort to reconstruct medi-

cine on the basis of the new discoveries in physiology and chemistry. It owed something to Paracelsus and Van Helmont, but took in the main an independent position. Its founder, Francis de Le Boë, usually called Sylvius (1614-1672), belonged to a French family settled in Holland. He graduated from Basle at the age of twenty-three, and was subsequently called to a professorship in Leyden. He was an eloquent lecturer and drew to the university a large number of students, but the system of clinical instruction, which he introduced here, and of which he is one of the fathers, renders his name worthy of the most honorable mention. He cultivated with enthusiasm physiology and chemistry, and was one of the first to acknowledge the truth of Harvey's discovery.

The novelty and simplicity of this system was calculated to win supporters, but it was bitterly fought in France by the partisans of ancient dogmatism. The growth of these systems shows unmistakably that the humoral pathology which had held an unbroken reign of nearly eighteen centuries was tottering to its downfall, and that henceforth therapeutics would become something more than an attempt to correct a fanciful acridity or viscidity of the various humors.

In addition to these sects the profession of France, at the period under discussion, was not only broken up into many factions, but between them a wordy war was waged with all the malignity and passion of doctrinal controversies. In France the new sciences rooted slowly. This came largely from the compact organization and power of the old Faculty of Medicine of Paris, which represented the ultra-conservatism of the age. Passionately devoted to the restoration of Greek and Latin medicine, it stubbornly closed its eyes to the light which was breaking along the medical horizon. It stood still while everything around it was advancing. Independent of the State for its existence or support, it was powerful enough to compel kings to treat with it and to

openly defy Richelieu. The story is told that Louis XI. wanted a copy of a manuscript of Rhazes, which was in possession of the Faculty, but permission was not granted until he had deposited security for the safety of the precious document.

Richelieu used his influence to persuade the Faculty to admit to the doctorate the sons of his henchman, "Gazetier" Renaudot,* whom the Faculty hated on account of his antimonial proclivities. The members firmly adhered to their privileges, and the cardinal was compelled to abandon his efforts.

The legal representative of the profession, with a corporate existence of more than 600 years, the Faculty, occupied the anomalous position of opposing every modern advance and of refusing to recognize any doctrine not hallowed by the seal of Galen.

It drove modern chemistry and physiology from its halls and forced them to seek refuge in the Royal Garden where they were taught by special order of the king. Here in 1673 Harvey's discovery received its first recognition in France, a half century after it had been given to the world, and by a singular coincidence in the same

*Theophraste Renaudot (1584–1653), a graduate of Montpellier, went to Paris in 1612, under the patronage of Richelieu, procured through the influence of his friend, Pere Joseph (L'Eminence Grise), the confidential secretary of the cardinal. Renaudot acquired the name of *Gazetier*, by which he is commonly known, from the fact that he edited "The Gazette," a paper published in the interest of Richelieu. He was a man of good abilities, untiring industry, ambitious and aggressive in pushing schemes, of which he had many. He was a violent antimonialist, founded pawnshops, intelligence offices, and a private hospital where clinical instruction was given for the first time in Paris. The partisans of scholastic dogmatism encountered no ruder adversary than Renaudot. He was able to defy the Faculty in its efforts against him while under the protection of his powerful benefactors, but fared badly after their death. The Faculty at once proceeded against him in the courts for his irregularities and he was almost ruined by the famous law suit of 1644 by which he was compelled to quit practicing in Paris on the ground that he was a graduate of Montpellier.

year in which Molière lampooned the anti-circulators with young Diafoirus in "*Le Malade Imaginaire.*"

The great chemical war which had come as a heritage from the previous century was now fought to a finish. This controversy is one of the most curious episodes in the annals of medicine. The opposition to the chemists, which dated back to Paracelsus, had now narrowed down to little more than a fight against the preparations of antimony.

The Faculty of Medicine solemnly resolved that antimony was "a murderous poison," and appealing to the secular powers procured parliamentary decrees in 1566 and 1619 prohibiting its use as a remedy under heavy penalties. The Galenists declaimed in unmeasured language against the poisonous qualities of chemical medicines, while the chemists ridiculed the inertness of the Galenical simples and condemned the abuse of bloodletting.

Angry discussions ensued, in which personal abuse, anonymous epigrams and harsh epithets atoned for lack of facts. Never did party spirit become more venomous. In the Faculty itself dissenting voices were occasionally heard, but they were speedily silenced and disciplined for their want of orthodoxy. Guénaut, Béda, physicians to Louis XIV.; Mauvillain, Bernier, Molière's friends, and many others, were publicly censured for prescribing antimony. "This made them return to their duty," says Guy Patin, who naively adds, "if hereafter they are wanting we shall not be."

In 1658 young King Louis lay seriously ill with fever at Mardyck. Doctors were summoned from all parts, Guénaut among the number. At a council, over which Cardinal Mazarin presided, it was agreed, on account of the failure of the routine bleeding and purging prescribed by Valot, to give a dose of antimonial wine. The king recovered, and Guénaut and antimony gained great glory. The antimonial faction now grew rapidly in influence and power, and in 1666 the non-progressive old

Faculty was compelled, in self-protection, to formally ask parliament to repeal the prohibitory decree of 1566, and thus ended the controversy which had run through a whole century.

Another notable feature of the medical history of the seventeenth century was the struggle of the surgeons for recognition by the physicians. The refusal of the physicians to admit the surgeons to fellowship on account of their barber origin gave rise to a controversy which, although centuries old, was carried on at this time with far more than the usual bitterness. In the face of this persecution, French surgeons led the world. Paré, Guillemeau, the Colots and others, had already enriched the profession by their learning and operative skill, and had won a reputation which will endure long after the Guy Patins and men of that ilk have been forgotten. France, indeed, was the only country which had a surgical college proper and maintained an entirely independent Faculty. The old Faculty did not reject surgery, but it excluded the surgeons. Louis XIV., as had kings before him, abandoned, after futile efforts, the idea of making the doctors associate socially with the surgeons. The latter still continued to designate their shops with the sign of three sacrament boxes supported by a golden lily, and were only allowed the bodies of certain malefactors for purposes of dissection. As late as 1660, the Faculty obtained a parliamentary decree forbidding the surgeons to wear a medical robe and hat, and revoking the privilege of the college of surgeons to bestow degrees. The government was at last forced to do for surgery what it had already done for chemistry and physiology. The Faculty closed its doors, the government opened others. The Royal College of Surgery was created. "This latter title," [title of Faculty] said Le Martiniere to Louis XV., "was the object of our ambition, but, since your supreme will has deigned to accord us the title of *College Royal*, the

honor of depending immediately upon your Majesty suffices to console us for every other distinction." It required the leveling influence of the revolution of 1789, in which the throne, feudalism and class distinctions went down together, to bring about the social equality of physicians and surgeons in France.

There are other phases of the professional history of this unique period which we must pass over. We have said enough to indicate the *embarras de richesse* which met Molière's search for medical incidents with which to illustrate his comedies.

We have next to speak of that passionate and honest genius, the wittiest physician since Rabelais, "a spirit all fire," as he himself says, who was so closely identified with the events just considered. Guy Patin (1601-1673) was a perfect type of the seventeenth century conservative. During his student and early professional life, he supported himself by correcting manuscripts and reading printer's proof. A fortunate marriage soon brought him riches and social position and the friendship of Riolan, the greatest anatomist of his age, to whom he acted as assistant, secured his professional standing.

Patin was a man of undoubted ability and learning; his Latin is remarkable for its purity, and completely disarms the witticisms of Molière against the Latinity of the doctors of that period. A wit, a brilliant conversationalist, a fascinating orator and writer, with a love of controversy that left no opportunity for the exercise of his powers unimproved, he fought his adversaries with the keenest sarcasm and maintained his own opinions with that dogmatism which always commands respect if not approval. His voluminous private correspondence, evidently never written for publication, is a rich mine of medical gossip and the chief source from which is compiled the inner medical history of the period from 1630 to 1673.

Patin was an active participator in the controversial disputes which raged around him. He combated the Ara-

bians, denounced the surgeons and apothecaries, rejected the Harveian circulation and chemistry. He hated antimony because it came from the chemists and cinchona because it was introduced by the Jesuits. He believed implicitly in Hippocrates and Galen, in the old Faculty, and in Guy Pâtin. His creed contained but two articles, bleedingand purging with senna. With a good lancet and a pound of senna he proposed to cure more patients than the Arabians with their polypharmacy. His love of bleeding was excessive, but in this he only reflected the orthodox practice of the day. He bled babes of three days and his mother-in-law of eighty years. He tells boastingly of a patient who recovered after seventy-two bleedings and forty purgings.

Boileau must have had Patin in mind when he penned the humorous line:

"The one died empty of blood, the other full of senna."

An erring medical brother, La Brosse, who died refusing to be bled, is thus charitably disposed of in one of his letters: "He did us the honor to call us sanguinary pedants, and said he would die rather than be bled. Die he did; the devil will bleed him in the next world, as a rascal and an atheist deserves." So much for not "dying according to form," as Molière more mildly puts it.

A zealous advocate of liberty and independence in civil affairs, Patin was most intolerant in medical matters.

His professional colleagues were all measured by a common antimonial standard, and those suspected of being friendly to the hated drug were denounced in unmeasured terms. Did the trenchant sarcasms of Molière, a mere playwright, wound as deeply as the biting irony and caustic criticism of Patin, an honored dean of the Faculty of Medicine and professor in the College of France? The pangs of the eagle, so the fable tells us, were not lessened by seeing that the arrow in her breast had been guided in its flight by one of her own plumes.

We now pass to a very brief analysis of the two come-

dies of Molière in which the medical situations are the most pronounced.

L'Amour Médecin, known as Molière's declaration of war against the Faculty, was written for a royal *fête* at Versailles in the fall of 1665. The author speaks of it as "a slight impromptu—a simple pencil sketch—sketched, written, learned and acted in five days." This play is a bold and audacious caricature of some of the most prominent physicians attached to the court of Louis XIV. The plot is extremely simple.

Sganarelle, a widower, out of purely selfish reasons opposes the marriage of his only daughter Lucinde to Clitandre, her lover. The daughter from grief and disappointment becomes melancholic.

Sganarelle, noticing the change in her health, endeavors to ascertain the cause, of which he pretends to be ignorant, but when on the point of uncovering a love affair craftily flies into a passion, refuses to listen to her pleading, and declares that she is an obstinate child upon whom love and reason are thrown away. At last, thoroughly alarmed by Lucinde's illness and by the representations of the cunning maid Lisette, who is determined to accomplish by intrigue what persuasion has failed to do, physicians are hastily summoned. "Not one but a lot of them," cries the frightened father. "One cannot have too many in a crisis like this." Four doctors answer the call. The maid recognizes one of them—Dr. Tomès—and a wordy battle at once ensues as to whether a coachman whom the doctor attended a few days before, is dead or alive. Dr. Tomès abruptly closes the discussion by stating with great solemnity:—"It is impossible. Hippocrates says that these diseases end only on the fourteenth or twenty first days, and he has only been ill six days." "Hippocrates may say what he likes, but the *coachman is dead*," cries the maid, as the doctor petulantly bustles into the consulting room. Comfortably seated, the physicians with a cough all around proceed to dis-

cuss not the symptoms of the patient, but the excellence of their mules, the health of the city, and questions of ethics. Just as they have reached the decision that a "dead man is but a dead man, and of very little consequence, but that professional etiquette neglected does great harm to the whole body of physicians," the impatient father bursts into the room and presses them for an opinion on his daughter's case.

They all begin to reply at once, and when the confusion of tongues has somewhat subsided, one is heard to advise a copious bleeding, which is condemned by another, who orders an emetic. A hot dispute ensues, in which extremely personal allusions are made to the deaths of former patients by bleeding and antimony respectively. The belligerents take a hasty departure, each assuring Sganarelle that the treatment prescribed by the other will, if followed, surely kill his daughter. Appealing in his distress to the remaining consultants, he is still more bewildered by their pedantic talk about fuliginous, conglutinous and mordicant vapors.

After outlining a treatment, they give the following guarded prognosis:

M. Mac. We do not say that your daughter may not die for all this; but you will, at least, have the satisfaction of having done something, and the consolation of knowing that she died according to rule.

M. Bahis. It is better to die according to rule than to recover in violation of it.

Thoroughly alarmed and perplexed, Sganarelle flies to the stall of a street-vender and purchases "*Orvietan*," which was, as has been already stated, the cause of Mauvillain's disgrace.* Its virtues were sung in

*About 1639, a charlatan began to sell, on the Pont Neuf in Paris, *Orvietan*, so named because it was prepared by a Doctor Lupi at Orvieto, Italy. His real name was Jacques Ovyn. His successor, Christopher Contugi, called himself "*Antidotaire du Roi*," and, according to Guy Patin, bribed twelve Paris physicians who had afterwards to ask pardon from the Faculty of Paris, to give him a certificate. M. Paully, in a note in the edition of Moliere published by

much the same language as are similar nostrums to-day.

Quack (sings).—

The gold of all climes which by the ocean are bound
Can e'er it repay this important secret?
My remedy cures, by its excellence rare,
More complaints than are counted up in a whole year;
The itch, the mange, the scurf, the fever, the plague,
The gout, the small-pox, ruptures, the measles,
Great powers possess my Orvietan.

At this juncture, the intriguing Lisette introduces Clitandre, disguised as a celebrated doctor who cures by talisman. The father is deceived; the lovers adopt by his consent and in his presence the talismanic marriage which turns out to be legal marriage, and thus happily ends the play.

L'Amour Médecin has a medical interest far beyond its literary merits, because of the introduction on the stage of famous living physicians, whose peculiarities were cleverly imitated by the actors, and whose very names were thinly hidden under Greek disguises framed, so tradition tells us, by Boileau.

It has been generally supposed that the four doctors held up to ridicule in the burlesque consultation scene were Daquin, Des Fougerais (Béda), Guénaut and Esprit. M. Raynaud gives good reasons for substituting the names of Valot and Brayer for Daquin and Esprit, and thus brings together the four physicians who were in attendance during the fatal illness of Cardinal Mazarin (1661). This celebrated consultation, which has passed into history as an *opprobrium medicorum*, is described by Guy Patin:

Yesterday at two o'clock, in the Bois de Vincennes, four doctors, namely, Guenaut, Valot, Brayer and Beda could not agree as to the disease of which the patient died. Brayer said the spleen was ruined, Guenaut said it was the liver, Valot maintained that it was the lungs and that there was

M. Lemerre, Paris, says *Orvietan* was an antidote of which the secret was communicated by Cardinal Deodati to his apothecary, Martin Guerche. It was then called *antietan*, which means antidote of the time.

water in the chest, while Beda affirmed there was mesenteric disease. Four skillful men!

An episode so grimly comical must have been a favorite theme of court gossip, and could not have been unknown to Molière. That the people fully comprehended the situation is clearly shown in the rude but suggestive compliment of the street carter to Guénaut: "Let him pass, let him pass. He is one of the doctors who killed the Cardinal for us."

Guénaut, who figured on the stage as *Macroton*—the drawler—in allusion to his slow and solemn speech, was the best known physician in Paris. For many years he had sustained medical relations with the royal family in some of its branches, and so great was his reputation that no man of quality could afford to be sick—much less to die—without calling him in consultation. He was an acknowledged leader in the antimony controversy, and was largely instrumental in bringing it to a successful close. Fond of show and innovations, he was the first physician in Paris to make professional visits on horseback. Contemporary writers speak of him as having been both avaricious and ambitious.

Guy Patin, who despised him as he hated antimony, likened him to a baboon and dandy, and constantly applied the most opprobrious epithets to him. Positive in his convictions and bold, even aggressive in their defense, he had many enemies, yet no man of his day wielded more power over his professional compeers than did Guénaut.

Brayer, who answers to the stage name of *Bahis*, hotly contested with Valot for the place of first physician to the king. He was wealthy and had high social position at the court. Singularly he is the only one of the four who receives honorable mention from Guy Patin, the medical censor of the period; Brayer probably did not give antimony!

Des Fougerais, who is mimicked under the name of *Des-*

fonandrès—the man slayer—was at the production of this play about seventy years of age. His real name was Elie Béda, which he exchanged for the more aristocratic Des Fougerais when he attained riches and distinction. "He was a worthy man," says Guy Patin, with characteristic irony, "who never changed his religion but to add to his fortune and advance his children." This was said in allusion to his conversion, under somewhat peculiar circumstances, from the Protestant to the Roman faith.

Valot, whom Raynaud assumes to be the original of *Tomès*—the bleeder, succeeded Vautier as physician to the king in 1652 on the payment to Mazarin of thirty thousand crowns.

The office carried with it great prerogatives. The holder ranked as Grand Chamberlain, with the hereditary title of count, and, in all matters of state medicine, had supreme jurisdiction over the doctors and apothecaries of the kingdom.

By order of Louis, who was very methodical in all of the affairs of life, Valot kept a daily journal of the king's health, which was continued by Daquin and Fagon, his successors. This curious journal, since published, records the prescriptions "with which heaven inspired him" to preserve the monarch's health. It is of value in giving us an insight into the practice of the day, and certainly tends to remove much of the dignity that doth hedge a king or a king's doctor. Valot's partiality for the lancet is sufficiently attested in this document to fully justify the application of the name Tomès—bleeder—to him. Another formality of professional etiquette insisted upon by Tomès would doubtless equally apply to each of the consultants:

M. Tomes. For my part, I am excessively strict on that subject except among my friends. The other day three of us were called to consult with an outsider; * but I stopped the

*A physician who had not taken his degree in Paris was called an outsider.

whole affair and would hold no consultation unless things were done according to form. The people of the house did what they could, and the case grew rapidly worse, but I would not give way and the patient *died bravely* during our dispute.

Dying bravely is a touch of genuine humor.

Valot, though old and feeble, insisted on following the king into Flanders when war was declared against the Low Countries, but soon succumbed to the fatigues of the campaign and died on the road (1671).

There is a fifth medical personage in the play who is supposed to represent the Faculty as a whole. Dr. Filerin seriously reprimands his colleagues for their unseemly quarreling in the sick-room, and pertinently asks:—"Do you not see the harm which these disputes do us before the world?" His platitudes are not prompted by selfish motives for "Heaven be praised my little affairs are already settled; whether it blows, rains or hails, the dead are dead and I have sufficient to be independent of the living." So far Dr. Filerin's professional morals are not very odious, but the satire of the whole is apparent when he says:

> The world for centuries has been infatuated with us. Let us not disabuse the public mind. We are not the only ones who live on the weaknesses of mankind. Flatterers, alchemists and astrologers speculate on and profit by the vanity and ambition of credulous minds. But the greatest weakness in men is their love of life and the source of our gain is their fear of death.

When his health was rapidly failing, and in the full consciousness of his approaching end, Molière wrote "*Le Malade Imaginaire.*"

This comedy is not a mere caricature of members of the profession, but the bitter protest of a dying man against the impotence of the medical art. The supposition that in the character of Argan Molière painted his own portrait, invests the play with a melancholy interest, the sinister presentments and passionate denunciations of which are but shadows of his coming fate. The Imagi-

nary Invalid portrays the character of a man who though in perfect health believes himself to be seriously ill, and who, terrorized by the fear of death, spends his days in analyzing his symptoms and in seeking remedies.

In checking the monthly statement of M. Fleurant, his apothecary, he makes the stupendous discovery that he is not so well as last month:—"This month I have only taken eight remedies and twelve enemas—last month I had twelve remedies and twenty enemas." To avoid such a lamentable occurrence in the future, he conceives the idea of marrying his daughter Angélique to Thomas Diafoirus, a medical student, son of Dr. Diafoirus and nephew to the great Dr. Purgon, Argan's family physician.

When pressed for his reasons, he says, "I want a medical son-in law and relations who are doctors, so as to have in my own family the sources of all the necessary remedies, consultations and prescriptions." The medical pedigree of young Diafoirus is good. Like the true seventeenth century conservative he "attaches himself blindly to the opinions of the ancients and would never understand or listen to the reasonings or experiments of the pretended discoveries of the age in reference to the circulation of the blood or other theories of the same kind."

Angélique has already pledged her love to Cléante, and looks with disfavor upon the aspiring and pedantic Thomas. The intrigue of the play is carried rapidly forward by the quick-witted servant, Toinette, the most perfect type of the soubrette among Molière's characters, and Béralde, the brother of Argan, a medical skeptic and probably the mouthpiece of Molière himself.

They endeavor to wean Argan from his insane hungering after doctors and physic, as well as from his blind confidence in Béline, his second wife, who awaits with equal impatience the death of the invalid and the possession of his property.

By a clever artifice the real feelings of the wife and daughter are exposed. Toinette directs her master to stretch himself out as if dead in his easy-chair. Dominated by the fear of death, that most tyrannical of passions, he shrinks from the thought, and starting suddenly from his half reclining position, whispers hoarsely, "Is there really no danger in counterfeiting death?" The wife on hearing the news expresses herself with such heartless candor as to infuse new life and new feelings into the pretended dead. The same stratagem shows such sincere grief on the part of the daughter as to force Argan's immediate consent to the marriage with Cléante, provided only that he will study medicine. Béralde now conceives the idea of Argan himself becoming a doctor, urging that the loss of a medical son-in-law would be more than counterbalanced by the "convenience of having within yourself all that you need."

This leads to the opportunity of introducing the well known burlesque ceremony of installation, which is absurdly comical and really a good parody of the graduating exercises of the medical colleges of that day.*

*It will be of interest in this connection to quote Dr. John Locke's description of the conferring of degrees at the College of Montpellier in 1676, at which he was present:—

"A procession in scarlet robes and black caps—the professor took his seat and after a company of fiddlers had played a certain time, he made them a sign to hold that he might have an opportunity to entertain the company, which he did in a speech against innovations—the musicians then took their turn. The inceptor or candidate then began his speech, wherein I found little edification, being chiefly complimentary to the chancellor and professors who were present. The doctor then put on his head the cap that had marched in on the beadle's staff, in sign of his doctorship—put a ring upon his fingers—girt himself about the loins with a gold chain—made him sit down beside him, that having taken pains he might now take ease, and kissed and embraced him in token of the friendship which ought to be amongst them."

Præses. Savantissimi Doctores,
Medicinæ Professores,
Qui hic assemblati estis;
Et vos, altri messiores,
Sententiarum Facultatis
Fideles executores;
Chirurgiani et apothecari,
Atque tota compania aussi,
Salus, honor, et argentum,
Atque bonum appetitum.

These lines are a part of the President's address, and are a fair sample of the mongrel Latin in which the whole interlude is written.

The candidate has a stereotyped answer to all questions about the treatment of diseases:—

Clysterium donare
Postea seignare
Ensuita purgare.

And in the failure of this treatment the only recourse is to:—"Reseignare, repurgare and reclysterisare."

Three times the Bachelierus Argan swears to keep the statutes of the Faculty, to adhere to the ancient rules and to employ only the old remedies.

The first draught of this burlesque, than which Molière never wrote anything more humorous, was sketched, says Raynaud, after a merry supper at Madame de la Sablière's, where were assembled the wits of the day, including Molière, Boileau, La Fontaine, Ninon de L'Enclos and probably the medical mentors, Bernier and Mauvillain.

Molière drew the outline and each member of the jolly company threw in some bit of detail. It was afterwards altered by Molière and adapted to its place in the play.

When Molière, as Bachelor of Medicine, was taking the oath in the fourth representation of the play he became violently sick. At the close of the scene he was carried to his home in the Rue de Richelieu, where he died in less than an hour, suffocated by a pulmonary hemorrhage. He had literally fulfilled the gloomy predictions so lately uttered, and died without medical assistance.

Begrudged priestly service by the church, he was buried at night, and now sleeps in an unknown grave.*

Read in the light of this tragic end, the bitter words which Molière, as Argan, utters against himself when urged by his brother to go to the theatre and hear one of the comedies of Molière, have almost a ghastly significance. These words may be accepted as Molière's medical confession of faith :

Argan. It is like him [Molière] to interfere in matters pertaining alone to the Faculty. He is a brazen impertinent fellow to make fun of consultations and prescriptions, to attack the body of physicians and to put on the stage such venerable persons as these gentlemen.

Ber. What would you have put there but the various professions of men ? They put princes and kings there every day, who are of quite as good family as physicians.

Arg. Now by all that is terrible ! If I were the physicians I would avenge myself of his impertinence and would let him die without assistance. He might say and do what he liked, I would not prescribe even the least bleeding or the smallest enema. I would say to him : die, die ; that will teach you another time to make fun of the Faculty.

Ber. You are very angry with him.

Arg. Yes. He is a foolish fellow, and if the physicians be wise they will do as I say.

Ber. He will be wiser still than your physicians, for he will not ask them for assistance.

Arg. So much the worse for him if he have no recourse to remedies.

Ber. He has his reasons, and he maintains that it is permitted only to the robust and vigorous who have sufficient strength to bear the remedies, but as for him he has just strength enough to bear his illness.

Perhaps one of the least objectionable medical characters on Molière's stage is M. Purgon, Argan's physician. We have in him a good portrait of the honest but misguided pedant—a type of character far more common in the seventeenth than in the nineteenth century. Béralde thus describes him :

* In 1792 certain remains, supposed to be those of Moliere, were removed to the Pantheon. It is now known that the wrong tomb was opened and his grave remains to this day unknown.

Your M. Purgon has no pretense about him. He is a doctor from head to foot; a man who believes in his rules more than in all mathematical demonstrations, and who would think it a crime to examine them; who sees nothing obscure in medicine, nothing doubtful, nothing difficult, and who with an impetuosity of prejudice, a stiff-necked assurance and a coarse common-sense reasoning rushes into purging and bleeding and hesitates at nothing. You must bear no grudge for whatever he may do; he would despatch you with the most implicit faith, and he would in killing you only do what he has done to his wife and children, and what, if need be, he would do to himself.

Most commentators think that the famous Fagon (1638-1713), who succeeded Daquin as first physician to the king in 1694, is caricatured in Purgon. The stinging accusation in the above quotation is similar to that made by Guy Patin against Guénaut, of having caused the death of his wife, daughter, nephew and two sons-in-law, by the use of antimony. There is, however, but little to sustain the opinion that the dramatist had either Fagon or Guénaut in mind when he drew the character of Purgon. Fagon had just received his degree from the Faculty of Medicine at the time the "Imaginary Invalid" was produced, and by reputation and works belongs to the latter half of Louis' reign. He owed his position at court to the second wife of Louis XIV., Madame Maintenon, between whom there existed a warm friendship. His learning, agreeable address, and fascinating conversational powers, together with his professional attainments, made him one of the most marked men of his day. It is said that he took no fees from patients, and abolished the perquisites that accrued to his office by governmental appointments, a pernicious practice which had grown up under the venal Mazarin. Fagon had the same love of botany, of which he was at one time professor, and the same feeling for the Royal Garden as did his uncle, Guy de La Brosse, its founder. His large contributions of plants and the fostering care bestowed on it, even when attached to the person of the King,

justly earned for him the title of second founder. He was one of the first of the Paris physicians to acknowledge the truthfulness of Harvey's circuit of the blood.

I have to-night purposely strayed from the great highway of professional thought into a little-used by-path of medical history. The medicine of the seventeenth century stands out prominently as one of the greatest landmarks in its history, and the story of its struggles and triumphs enables us to better understand the present and to more accurately forecast the future.

We fortunately belong to a century which has illumined every department of human thought with learning and genius, and which has done so much for the upbuilding of scientific medicine. But we must not forget that each preceding age has wrought its full share in the evolution of medicine. It fell to the seventeenth century to break the chains of servitude, and liberate medicine from the trammels of authority which had so long hindered the coming of modern science.

To us it seems almost incredible that one of the most difficult lessons the race had to learn was to accept the testimony of the senses, and yet every page of history attests its truth. It is precisely here that we find the difference between ancient and modern medicine. The one concerned itself only with the proofs of authority; the other relies wholly upon the proofs of experiment and experience. Modern medicine acknowledges no intellectual restraint. She bids defiance to tradition, boldly questions authority and sets precedent at naught. With the present means of inter-communication and interchange of ideas, which bring into elbow-touch co-workers in every corner of the globe, the day of long probation for alleged discoveries has gone forever. Illustrations crowd upon our attention. The Elixir of Brown-Séquard and Tuberculin of Koch, both too recent to need elaboration at our hands, strikingly illustrate the modern method

of dealing with new problems. Every new hypothesis is at once submitted to fair but rigid laboratory and clinical tests, and by the result of these must it stand or fall. But opportunities bring with them responsibilities. A past, glorious in its achievements, warns us to avoid the errors of the epoch which we have been considering lest another Molière should come gifted with the power to make us "See oursel's as ithers see us," and a future, splendid in its possibilities, lays upon us the obligation to so labor that the profession of medicine shall continue to advance with a broad front to greater victories.

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THE SURGICAL TREATMENT OF CHRONIC CATARRHAL APPENDICITIS.

R. HARVEY REED, M. D. (UNIVERSITY OF PENNA.), MANSFIELD.

In searching for the practical uses of the appendix vermiformis, I am constrained to repeat the old adage,

"Everything can something do,
But pray of what use are you?"

Unless it is to get up a disturbance with humanity, and give employment to the surgeon and the undertaker.

We are told that the only mammals which are so fortunate as to have escaped this rudimentary conclusion of a once lengthened cæcum are the orang-outang and the wambat, excepting an occasional *anomalia* found in the human family who has fortunately got beyond this rudimentary decoration.

So much has been said and written about this useless appendage in the last ten years, that I shall not take up your time discussing its anatomy and anatomical relations, nor in trying to find an excuse for its existence by attempting to hypothesize some imaginary, trivial benefit it might be, under certain circumstances, in aiding the process of digestion, but will proceed at once to the consideration of the surgical treatment of chronic catarrh of this distinguished appendage.

Whilst catarrh in the more limited sense of the word is intended to indicate a flow or discharge from a mucous surface, according to the older authors, yet by the more recent writers it has been given a wider scope, and consequently it has been applied to general inflammations of

the mucous membranes, which of course may be either acute or chronic.

Again, whilst catarrhal inflammations of the mucous membranes are usually accompanied by more or less discharge, yet we may have a chronic inflammation of a mucous membrane with little or no discharge, in which case there is usually an atrophic condition of the membrane. Usually, however, there is hypertrophy of this membrane with an excessive mucous or even muco-purulent discharge, which is not infrequently associated with a circumscribed necrosis of the mucous membrane, which in chronic catarrhal appendicitis frequently extends to and may even involve the serous coat of the appendix, producing a complete perforation.

The peculiar location of the appendix vermiformis, associated with its anatomical construction and relations, predisposes this particular portion of the digestive tract to chronic catarrhal inflammation more than any other one portion of the intestinal canal.

Some one has said that the absorbent properties of this rudimentary attachment were greater in proportion to its size than any other portion of the alimentary tract; and hence the liquid portions of the contents of the intestine which found their way into the appendix were more rapidly absorbed and therefore left a concrete mass which becomes a foreign body, and sets up an inflammation of the mucous membrane which soon becomes chronic.

As to the veracity of this statement, I am not prepared to give any definite opinion, but it seems to me that owing to the small diameter of this appendage, which in addition is closed at its most pendent portion, and which opens above into the cæcum, from which gravity alone will promptly supply it with both liquids and solids at all hours of the day or night, it is easy to account for the formation of concretions in this appendicular attachment.

Under the circumstances there must be more or less stasis of the contents of the appendix vermiformis, which

ordinarily must be emptied of its contents by a sort of a *vis a tergo* process, which we must all concede is not very active.

Consequently, owing to the imperfect circulation of the contents of the appendix and the continued absorption of the liquid portions therein, it is easy to account for the formation of these concretions which are liable to be followed with chronic catarrhal inflammation and ulceration of its mucous membrane, which so frequently result in perforations or acute suppurative inflammation which not only threatens the patient's life but often destroys it altogether. In addition to these concretions which are found so frequently in the appendix, it is liable to be invaded by cherry pits, grape seeds and the like, which have the same effect on it as the concretions which may accumulate in it from the contents of the bowel.

Again, we may have a chronic appendicitis following an attack of acute enteritis or even an attack of peritonitis, metritis, salpingitis or ovaritis, which often results in adhesions of the appendix to some of these parts. It is only a few weeks since I operated on a case, where I found the appendix vermiformis firmly attached to the fundus of the uterus and which was greatly enlarged and thickened by chronic inflammation.

Again, I have found it adherent to the broad ligament, the fimbriated extremity of the Fallopian tube, and even the ovary.

The next important question is, how shall we determine the existence of chronic catarrhal appendicitis with practicable accuracy?

We must all admit that simple acute appendicitis or suppurative perityphlitis is easily diagnosticated; but when it comes to making an accurate diagnosis of chronic catarrhal appendicitis, it is much more difficult. It is true, that many cases of chronic catarrhal appendicitis fail to come under the notice of a surgeon until they are suffering from an exacerbation of their chronic trouble, or an

acute attack of appendicitis which is threatening life. Yet, when these chronic cases do present themselves to the surgeon during the intervals of their recurrent attacks of acute inflammation, then is the time for the surgeon to determine the exact nature of his case and follow it with practical and timely advice, and not wait for an inflammatory eruption to occur before he decides what it is, or what he will do for it.

The point was well taken by Fitz when he claimed that usually the primary causes of every perityphlitic abscess were the result of a perforated appendicitis (and we might add) or a chronic inflammation of the appendix which so frequently results in perforation sooner or later.

This being true then, it is easy to see the importance of making a clear diagnosis of a chronic catarrhal inflammation of this rudiment of the cæcum as early as possible. In order to do this you will find it important to ascertain the condition of the bowels; for if you have constipation alternating with diarrhœa and accompanied with circumscribed tenderness over the region of the right iliac fossa, it is well to be on the lookout for a case of chronic catarrhal appendicitis, and especially so if there is a history of having had an attack of acute perityphlitis. If the case is a woman, you may be able to feel the thickened appendix by placing one of the index fingers well up in the right horn of the vagina whilst you palpate over the region of the appendix with the other, excepting in cases where the abdominal walls are very thick and firm.

In this class of cases you can easily distinguish between the ovary and the appendix by remembering that when you squeeze the ovary it produces a sickening sensation, whilst pressing on the inflamed appendix produces only pain without this peculiar sensation of nausea.

In the male there is no occasion for making a diagnosis of this character. The very fact that you have a circumscribed tenderness over the region of the right

iliac fossa is sufficient evidence, barring the bare possibility of a hernia, to arouse your suspicions.

If the irritation is very marked, you may have vomiting as well as constipation with all the symptoms of intestinal obstruction. I have seen numerous cases of this character when there was every apparent evidence of obstruction to the intestinal tract, but which in reality was not obstructed at all only so far as the inflammation of the appendix extended to the muscular walls of the cæcum and produced temporary paralysis resulting in obstruction.

I recall one case in particular which I reported some years ago, in which there was a strangulated hernia of only the appendix vermiformis, in which there was every symptom of occlusion of the bowel, which was relieved by an operation just as promptly as if we had had a real obstruction of the intestines. I speak of this to show you that inflammation of the appendix will produce constipation and even stercoraceous vomiting just the same as in complete obstruction of the bowel.

In the severer forms of chronic catarrhal appendicitis the pain may occasionally be referred to the umbilical or epigastric region. Again, the patient, if a close observer, may notice mucous or muco-purulent discharges attached to his stools, which of course might come from some other part of the intestinal tract, but if he has circumscribed tenderness over the region of the appendix and no marked tenderness elsewhere along the alimentary canal, it is reasonable to conclude that it is more probable that it comes from the appendix than elsewhere.

Again, if a patient comes to you complaining of either gastric or intestinal indigestion or both, accompanied with a circumscribed tenderness over the region of the right iliac fossa, look out for a case of chronic catarrhal appendicitis.

In a patient where the abdominal wall is very thin and the appendix is very much thickened, you may be

able to trace it by a careful digital examination associated with percussion.

Rectal examinations are only beneficial for obtaining negative results; for instance, a patient is having mucous and muco-purulent discharges associated with the stools, and it is a question whether the mucus comes from the rectum or higher up, then an examination may decide the question which previously may have been one of doubt.

We have already spoken of attachments which may occur in the female between the appendix and the uterus or some of its appendages; of course in the male these can all be excluded, but we may have omental attachments, which are common to both.

I remember of holding a post-mortem a few years ago on a physician who died of rupture of the heart, who, during life, had frequently referred to a tumor in the right iliac fossa, and who had frequent attacks of gastric and intestinal indigestion, which the autopsy revealed to be undoubtedly the result of chronic catarrhal appendicitis, as we found the appendix attached to a roll of omentum, no doubt the sequela of chronic inflammation.

You will readily concede that we might have the same condition of affairs regarding attachments of the omentum and appendix in the female as well as in the male.

Having cleared up your diagnosis and fully decided that you have a case of chronic catarrhal appendicitis, the next important question that confronts you is, how shall we treat it?

Of course, the timid and uncertain doctor will always advise the expectant treatment, and is ever ready to "wait for something to turn up," which advice the *unsuspecting* patient is usually very ready and willing to accept.

Arnica, poultices, liniments and blisters form the chief weapons of *external* "warfare" under these circumstances, whilst "blood medicine," "liver pills" and

"tonics" are given to fortify the *inner man* against the invasion of this subtle foe.

The patient gets better and worse, alternately, from time to time, until finally a crisis comes. By this time, however, the catarrhal inflammation has done its work, and a perforation has taken place, and the result is an attack of perityphlitis. Acute, circumscribed, or even general peritonitis has set in, and death is staring the patient in the face. A surgeon is sent for, but it is too late, and the undertaker *completes* the work of expectancy. If, however, nature comes to the rescue and throws up a fortification, so to speak, of plastic material around the perforation, and confines the escaping contents of the perforated appendix to a limited area, then you will most likely have a perityphlitic abscess, which the surgeon's knife may relieve and permit your patient to get well, with a lot of peritoneal adhesions and general thickening around the end of the cæcum, which may possibly involve the ileo-cæcal valve, and lay the foundation for a case of intestinal stenosis at some time in the future.

In illustration of the above I recall a case, to which I was called in counsel a few years ago, of a young man whom I found dying from acute peritonitis, which I diagnosed as a result of a perforation somewhere in the region of the appendix.

A post-mortem confirmed my opinion, and revealed a concretion in the appendix which had set up a chronic catarrhal appendicitis, followed with perforation, general peritonitis and death, in proof of which I will ask you to examine the specimens which I obtained from this case, and which I will now pass around the room for your inspection.

In the same connection I recall another case to which I was called a few hours before death, which the post-mortem revealed to be a case of acute peritonitis, the result of a perforation of the appendix from chronic

appendicitis. In this case, however, I was not fortunate enough to secure the specimen.

I might go on multiplying these cases, but that would only be occupying your time to no advantage, and would prove nothing more than I have already proven; for as has been said, "by the mouth of two or three witnesses, every word shall be established."

There is no question in my mind but that a timely amputation of the appendix in each of these cases would have saved their lives and secured them health and comfort for years to come.

It was my good fortune on one of my visits to Professor Senn's clinic, at the Milwaukee Hospital, to see him examine a case and diagnosticate chronic catarrhal appendicitis, and witness him operate the same case and remove from a young man a thickened appendix, the result of chronic inflammation, the walls of which had almost ulcerated through in several places, producing a perforation which no doubt would have occurred in a short time, had he not been operated upon and the appendix amputated.

In answer then to my question, "How shall I treat these cases?" I will reply: By all means amputate the appendix just as soon as you are positive of the existence of chronic catarrhal inflammation of the same. Don't wait for something to turn up—those days are gone by; be accurate and active, and don't wait for the undertaker to come to your rescue and bury the results of your procrastination beneath the sods of the valley. The operation is an easy one and not attended with any marked danger to life, if properly performed under strictly aseptic precautions. In fact, the patient assumes more danger every day he lives harboring a difficulty of this character than he does in having it removed by an operation. Again, you have the satisfaction of knowing, that like a sheep's tail, the appendix is neither useful nor ornamental, and the sooner it is cut off the better.

In amputating the appendix, it is only necessary to make an incision over the region of the appendix parallel to Poupart's ligament just large enough to expose the end of the cæcum and its rudimentary attachment. Then ligate the appendix as close as possible to the cæcum with an aseptic silk ligature, nip off the offending rudiment with a pair of scissors, cover the stump carefully with a hood of the omentum which should be held in place with a few aseptic silk stitches, make your toilet carefully and close up the external wound and cover it with an antiseptic dressing; place your patient in bed and keep him there until the parts are thoroughly united and have regained strength enough to resist the tendency to ventral hernia.

When this operation has been successfully performed, you have removed the ax, so to speak, which was hanging by a frail thread over the head of your patient and daily threatening his life. You have given him a new lease of life with an extended expectancy of the same, and made a patient who was once uninsurable a fit subject for insurance, saying nothing of the comfort, both mental and physical, you have afforded him by the operation.

Then, as Solomon says, "the conclusion of the whole matter is:"

1st. That the appendix vermiformis is a useless rudiment of the cæcum.

2d. That chronic catarrhal appendicitis is always dangerous and liable to be followed at any time with hazardous and even fatal results.

3d. That medical treatment is of little or no permanent value in this class of cases.

4th. That the only safe and reliable method of treating chronic catarrhal appendicitis, and the only treatment that will promise permanent relief to your patient, is prompt amputation of the appendix.

DISCUSSION.

Dr. M. Stamm.

I would have liked if Dr. Reed had said more about the diagnosis of these cases. A great many say we ought not to be so rash in operation; that a number of cases will get well without operation, after a year or two of trouble. I think we ought to distinguish in point of diagnosis in such cases. We have not simply chronic catarrhal appendicitis, we have an acute form of appendicitis, which, in my opinion, is the most dangerous; and generally, when we operate on those cases they *leave*. I made that remark in Washington at the last meeting. It seemed to me that the distinction was not made between these different forms. Dr. Fenger, of Chicago, read an article by Mr. Leaming, of New York, which, in my opinion, is the only classical article ever written. It substantiates my opinion that they can be diagnosed. You have no pain in the iliac region. They generally complain of pain in the stomach, and somewhat in the right side. I had two cases where the pains were in the stomach and in the region of the ilium. I think where that is the point of pain, where the patient has had pain for months—recurring pain for months and years—where we feel a tenderness and sometimes a dullness, I think we ought to give that patient all the benefit of an operation. But we have not always a prominent point. Sometimes you have a pain in the lumbar region. Lange reports a case where a woman had a pain in the appendix two months after confinement. The diagnosis was peritonitis. He delayed, and didn't cut down for some time. At the time of the contraction of the uterus it adhered to the cæcum. He operated on two cases of acute appendicitis. In one case there was no perforation, and in one case there was blood and fecal matter. It was not peritonitis due to inflammation; I think it was toxemia. And Lange speaks of this case, too. It is simply due to toxemia. Those cases ought not to be touched by the surgeon. I would like to hear the old practitioners talk about the diagnosis of such cases—men who have been observing these cases for years and years—and give us surgeons a little hint on the subject.

In point of operation, it is a very simple thing in a case of appendicitis, where there are not many adhesions; but sometimes you find a case of manifold adhesions, so

that you can't reach it from the semilunar region in the ileo-cæcal region; sometimes you have to go into the lumbar region. Lange had to operate through the ilium, and I think in one case he had to puncture through the vagina. There are cases where you can't remove the appendix. You can remove the fecal matter contained in the appendix; all you can do is to scrape it, or to put in a tampon. Lange had a case where the patient had been suffering for a year and a half. He had some dullness and pain in the ileo-cæcal region. He cut down and found constriction of the cæcum by adhesions formed. He loosened that. About two months afterward he found a fecal stone, and removing that, he cured his patient.

Dr. C. A. L. Reed.

This paper of Dr. Reed's should elicit the discussion which the importance of the subject warrants. I believe with Dr. Stamm, that the important question involved is the question of diagnosis. When once diagnosis is unequivocally made, I would say that our duty is clear and imperative, and that duty is in the direction of operation. I noticed with some care the diagnostic points that were enumerated by the author of the paper, and I confess that they did not impress me as being conclusive.

I have learned to look upon recurrent pains, with associated tumefaction, in the ileo-cæcal region, as being indicative of chronic catarrhal appendicitis, and as being an indication that operation should be resorted to; but I have since been convinced that this aggregation of symptoms is not conclusive. Not many months since my friend Dr. Fackler referred a case to the Woman's Hospital for operation. My colleague, Dr. Hall, I think, was present at the time when I operated upon the case. There was presented the history of pronounced pain, repeated and continuous, localized tumefaction, in the region of the vermiform appendix. I found in that case, not the vermiform appendix that was involved, but an enormous pus-tube that was adherent to the meso-colon. That accounted for the physical symptoms which had led previously to the supposition that the patient was the victim of trouble at the vermiform appendix. Of course, we found ample justification for an exploratory incision—indeed, for a complete operation; but the completed operation was not the operation which we first had in mind.

The physical symptoms, by the bimanual examination even, in women are not very satisfactory as they relate to the vermiform appendix. I am now speaking of the vermiform appendix when it is simply the seat of catarrhal inflammation—not when it accompanies an abscess, not when we have McBurney's point, but when it is catarrhal.

In some of my recent abdominal sections I have included the vermiform appendix in the same ligature by which I tied up the tube, and found often clearly and unmistakably that the vermiform appendix was the seat of inflammation. The first time was by mistake, when I happened to take in the vermiform appendix; and at the time I found it was the seat of catarrhal inflammation. And in subsequent operations I have often found it the seat of catarrhal trouble. I think in four or five cases I have done this incidentally, but if I had made the diagnosis without the exploratory incision, I could not have properly diagnosed the trouble. The condition was masked by the course of the disease in the uterine appendages.

We are speaking now, as I understand it, simply on catarrhal inflammation. That is a form of disease which, in my opinion, will justify operation, because it is a menace to the patient, and a serious menace. A patient with catarrhal appendicitis is simply walking around with a dynamite magazine, which is liable to explode at any moment. I shall be obliged to Dr. Reed or to any person who will give any definite symptoms by which I can tell when a patient has this form of disease. I will be thankful for a definite symptom which will justify me in making an exploratory investigation.

Dr. W. J. Scott.

I want to express a notion that I have. I have seen these cases a number of years, off and on, and, as I tell surgeons, I would like any man to point me out and state the symptoms from which he can absolutely say that it is necessary to make an operation. How many times in my experience have I seen cases of this sort, involving inflammation of these structures, with typhlitic condition, or perityphlitic condition, get well without an operation? I know, as well as Dr. Reed, that this subject is at the present time occupying the best surgical attention in the world, and those who think about the subject teach us of

the difficulties we have in making the diagnosis. I cannot myself say that it is necessary, to save the life of this patient that I have before me, to make the operation; but often, in the acute stages, as the gentleman has stated, I would say, wait until something more certain develops before I would advise even an exploratory incision, because I know from personal experience that in many of these cases the patient will get well without that. When it becomes a chronic disease, when it becomes a suffering disease, with marked fluctuations, why then the surgical operation is necessary, but in the acute stage I deny that that is true. No practicing surgeon in New York that I am acquainted with advises to-day to follow this course; but, on the other hand, some of the best men in the profession claim that we cannot always make so positive a diagnosis as is necessary to make even an exploratory incision. That is dangerous ground. On the other hand, many of these cases, by waiting a little bit, by giving the patient an opiate to control the pain, waiting for further developments, get well without the operation, and get well better than with it.

Dr. Edwin I. Ricketts.

The paper did not touch upon the subject of opium in the treatment of these cases, which the gentleman last on the floor has just mentioned. This subject of catarrhal appendicitis is one of the greatest importance. These cases are more frequent, perhaps, than some of the conservative men would have us believe. As to the manner of arriving at an early diagnosis in this particular class of cases, no one will say that it is an easy matter to say just when to open the abdomen, and just when to push on in the light of medical treatment. I will say this, that whenever there is a suspicious mass over the region of the vermiform appendix, and you are in doubt as to what the trouble is—as to the exact trouble—then an incision by experienced hands is an aid to diagnosis. It is not a dangerous procedure. It will open the door, and with the aid of the senses of sight and touch you will be able to make a diagnosis in the case, and go on and do whatever is necessary. It is one thing to open the abdomen, and it is another thing to know when to stop after opening the abdomen.

One word as to the use of opium: I want to say here, gentlemen, that the hypodermic syringe that goes abroad

in this land, in many and many a case locks the door, and puts that patient in a condition that he cannot tell you what is the matter. It many a time masks the symptoms that are so important in arriving at a diagnosis.

In other words, whenever you are in doubt as to the condition on the inside, make your exploratory incision, and then do not make an unnecessary operation.

Dr. D. N. Kinsman.

I can only speak from the standard of a simple practitioner of medicine upon this subjeet. In the first place, I do not believe the concretion is very hard in the inside of these tubes, even by chronic catarrhal appendicitis. The character of that formation, when you come to examine it, is not made of epithelial débris in the greater number of cases. Some of these I have examined—not many. It is beyond my power to diagnosticate either acute or chronic catarrhal appendicitis. I don't know what I might do if I was a surgeon. Catarrhal inflammation of any part means a superficial inflammation, with an increase of secretions of the surfaces. There is no violent disturbance. In appendicitis of this nature there is no severe pain—any more than there is in catarrhal inflammation of the small or large intestines. Pain in these organs is excited by the presence of other material than the catarrhal products. So in appendicular colic the pain is caused by the intrusion of material from above into the appendix. The material enters, and it may be forced out again. While present there is pain ; when it escapes the pain ceases. The same thing occurs from the intrusion of calculi into the ureter. The calculi can go through ; the concretion must back out. When they back out the colic ceases, and this may and does occur often in the same case, without any inflammation. It may be catarrhal appendicitis, it may be appendicular colic. In thirty years' practice I have seen a hundred such cases get well without surgery. I have seen some die.

Dr. Reed :

How many ?

Dr. Kinsman :

I can recollect three or four within the past year that have died under my observation.

Dr. Reed :

None got well ?

Dr. Kinsman:

Foreign bodies, gangrene, tubercles and, perhaps, other causes lead to perforation. I have known two cases in which the abdominal cavity was full of pus, so that when the section was made the pus squirted to the ceiling, and yet no diagnosis could be made, and the true condition was revealed by the operation. I criticise the paper for its method of diagnosis. I criticise the wholesale incision of the abdominal cavity, no matter what the condition is or what the supposed difficulty may be, for diagnosis.

You may have a case of bellyache, for three days or three weeks. Cut into the belly and find out what causes it. Persistent pain is the indication for it, no fever, but you find a lump in the groin. Its very nature may be settled by an operation, and you cut upon it. The recuperative power of nature is forgotten, her assistance rejected. This is not right, and I believe the surgical section of the country is returning to common sense treatment of this subject. The paper of Dr. Joseph D. Bryant, and the contribution of Lenoir, are evidences on this subject. It makes all the difference in the world whose belly you are cutting into.

Dr. Rufus B. Hall.

The last speaker excited me a little bit, and I will talk a few minutes. The essayist did not offer a diagnosis for acute appendicitis, and I hardly think it is fair to take that up in discussion. I am very strongly inclined not to operate in acute cases. As we all well know the majority of these acute cases recover, and recover without any serious trouble. But, sir, it is in the cases referred to by the essayist, recurring attacks of inflammation, that we ought to watch. They are cases that bear watching. If we have it recurring two or three times, we may have it again and again until pus forms and the patient dies from perforation. I doubt my power to diagnose chronic appendicitis before exploration. I don't believe I can do it; I never have done it. I may learn later to do it. The point that impressed me, and the point that we ought to bear in mind, I think, in this affection, is this: that with these recurrent attacks the patient's life is jeopardized, and that is the time for an exploration. I mean by that, if we have not one, but two or three recurrent attacks, I think that, with the McBurney point and the dullness on percussion, is an indication for an exploration. I want to

put myself on record upon this point. I hear some men say that even with two or three, or four recurrent attacks they don't think that it is absolutely necessary to operate even then. I believe that in some of these cases a permanent recovery is had, even after three or four attacks of appendicitis. I can refer to more than one gentleman here who has had more than one patient that has had recurrent attacks and recovered. But there is this always that we must remember: that these patients are in danger, after the second attack, to go off by suppuration. I have seen in my own work—I don't know whether that holds good or not; I have not seen it referred to—that if the recurrent attacks are more mild each time, those cases are usually the ones that recover; if the attacks are more severe than the previous ones, those are the ones that usually go on to suppuration. If I were the subject of chronic appendicitis, and there was a question of doubt, I would not have an exploration; I want to give the patient the advice that I would take myself. But when to have an operation, that is the question. Of course, that is a very nice point—just when to operate in these cases. It has been said by one gentleman that when we feel a lump in the right side, that is the time to make an exploration. I differ from that gentleman. I think, as said by one gentleman on the floor, that in acute cases we can do harm by an exploration.

Referring to the use of opium, I think the use of opium puts the physician at a disadvantage in the matter of diagnosis. I would much prefer to clear out the alimentary track with salines and let the man stand his pain, and he can tell us how much pain he suffers. I don't believe that opium cures peritonitis. I don't believe it prevents peritonitis.

Dr. J. F. Baldwin.

The discussion seems to take a broader ground than the essayist's paper, which is on chronic catarrhal appendicitis, and yet acute appendicitis is discussed. Dr. Kinsman looked over in my direction during the course of his remarks. I think it behooves me to say something on this subject, speaking from my own observation and personal experience. We had a case in Columbus of this acute suppurative appendicitis, a young lady in charge of two young physicians. Dr. Loving and Dr. Kinsman and myself were called in consultation. We decided that no

operation should be made. One of the young men was inclined to operate. No operation was made. In the course of three or four days we had the post-mortem. We found such a mass as the essayist showed us, and that caused a suppuration of the appendix. It was loose in the abdominal cavity, a general peritonitis, stinking and rotten everywhere. The young lady died without the benefit of an operation.

A week or two after that another young physician in town had a case of stomach ache, and I saw the case one evening with him. I said there was a general peritonitis, that the only chance to save the boy's life was an operation, and that was a very poor chance. The parents, of course, would not consent with that amount of medical ability, and therefore the next morning Dr. Loving and Dr. Kinsman, myself and Dr. E. J. Wilson saw the case. As I had already expressed my opinion with regard to the treatment and prognosis, I had nothing to say. These three gentlemen said operate; that was the only chance to save the boy's life. Mark you, they said there was not yet a general peritonitis. The pulse was about 80, a calm, quiet, easy and even pulse. His temperature was below 100. Not much tenderness. No great tympanites. The same day the result of the consultation was presented to the family, and they said then to have the operation performed and pitched upon me to do it. An hour or two afterwards, as quickly as I could get ready, I performed the operation. The first cut of the knife in the abdominal cavity revealed the pus. There was general suppurative peritonitis everywhere. Just look at what the symptoms were; pulse 80, quiet, calm, full, easy pulse; skin in good condition; the boy's general condition apparently excellent. There was then peritonitis, although there had been no thought of the appendix, because his first complaint was of cramp in the stomach. After that had passed off he went to church; no, he went to a theatre on Sunday evening. He came home from the theatre and was taken into a doctor's office with a severe pain, a simple cramp. There was no tenderness, no McBurney point. There was no lump at this point, and no special tenderness at this point. As soon as we got the pus sponged away, I went at once to this point, as almost anybody would, and found acute appendicitis, and removed the appendix. There is use in operation in those

cases. I removed the appendix, cleaned out the abdominal cavity and sewed up the external wound.

Dr. Reed:

How long was the boy sick?

Dr. Baldwin:

About six days. He got up and walked about his room Thursday morning, and took a glass of ice water. I saw him Thursday night. The operation, I believe, was performed Friday morning.

The second case I was asked to see in consultation was an old man with a hernia which his attending physician had been unable to reduce. I found that he had, as his physician said, an attack of general peritonitis two weeks before, from which he had recovered. He was a man who was very fat, immensely fleshy. I succeeded in reducing the hernia. The man was in bed. It again came down in the course of twenty-four hours. I was sent for again. I could not reduce it. I urged an operation. They would not listen to it. The next day I saw the patient again. Again we tried taxis, failed to reduce it, and again I urged an operation. The third day they still refused an operation, until in the night stercoraceous vomiting came on, and then they consented to an operation. I hoped to get the organs and mass back into their proper places. I opened into a pocket of pus, off in this region, a large mass, stinking most terribly. I found in that a little hardened concretion, which was the cause of that abscess. I cleaned out the cavity and put in a drainage-tube. The man simply went right on down as he had been doing for thirty-two hours. The operation didn't affect it in one way or another. He died. No post-mortem was allowed, and we didn't go in there. There was a case of acute suppurative appendicitis, producing, not, I think, a general peritonitis, the symptoms didn't warrant it, but a local peritonitis, and did that without being recognized as such by a very intelligent physician, who had charge of the case during two weeks.

I have seen, as Dr. Kinsman has said, a number of these cases every year. I have seen a good many of these cases get well; and this is true. Further, I don't know much about the chronic catarrhal form. I don't know about the diagnosis of recurrent appendicitis. I don't think there should be any question as to operation in recurrent appendicitis; but in these acute cases the

question is, how shall we know in what cases we shall operate and in what we shall not? Here are these three cases that I have mentioned in which the symptoms did not seem, during the first six days, to point to an operation, and yet in all those cases an early operation would undoubtedly have saved life.

We discussed this matter a month or so ago in the Central Ohio Society. I recalled twenty or thirty cases that I have seen in three years of this form of trouble. Out of that number I remember three or four deaths, not counting these last deaths. I think it is safe to say that of those cases that I have seen in my own practice and in consultation at least one-half have died, or 50 per cent. Suppose that all the people in this room were to be submitted to the removal of the appendix by competent operators, how many deaths would result? Probably not over 2 per cent. Putting it in the personal equation, as Dr. Hall did, I think if I had an attack of acute appendicitis, and there was near at hand an excellent operator in abdominal work, I should take my chances to have an incision made and that appendix removed, rather than take my chances of getting well with opium. I am not prepared to say that for the general patient. I don't believe in cutting too much; but if it was my own case, I think my chances to get well under opium would not be over one in five; I think my chances to get well after laparotomy, properly performed, would be ninety-seven or ninety-eight in one hundred, taking the statistics of reliable laparotomists, because, I am sorry to say, they are not all reliable.

Dr. T. W. Jones.

I am able to give testimony, being a subject myself, some two years ago in February, when I was attacked with what was diagnosed by my friends Dr. Kinsman, Dr. Gay, Dr. Hamilton and some other physicians, as acute appendicitis. I will say that my case disappeared; of course, it was some days before it disappeared. When the time came the knife was presented to me. Dr. Hamilton and his son came expressly to use the knife on me; and yet I am thankful that I was spared the knife, also that my life was spared up to this time. I had one of that character under discussion. I simply relate this that you may make the application as to the usefulness of the operation.

Dr. C. A. L. Reed:

I would like the consent of the society to say just a word. I want to speak with reference to a remark made by Dr. Kinsman. I am not a radical operator in these cases. I want to have something to say before we proceed. [Leave having been granted, Dr. Reed proceeded.]

A reflection was made upon the general practice of abdominal surgery, that it had been too extreme, and that it was perhaps approaching rational grounds. That extremes did occur in the early development of the practice I believe is true. In every development that is true. That it adjusts itself to a rational basis sooner or later is, I believe, likewise true. No one will believe that in the treatment of this particular class of cases abdominal surgery is irrationally extreme. I do believe that medical practice has been guilty of irrational conservatism; and the case alluded to as a forcible illustration, by my friend, Dr. Kinsman, is a case where the patient had been killed by the medical treatment. I should say to let a patient lie until the abdomen becomes full of pus is, if I may use a mild expression, a reflection upon that practice which justifies procrastination. What I want to do is to urge that the general practitioner meet the abdominal surgeon in the march of progress. I want Kinsman and his kind to get a move on themselves.

Dr. R. H. Reed:

Those of you who heard my essay will notice that I treated of chronic catarrhal appendicitis. I am glad to aid in bringing up this subject, which you must all confess is a subject that has been treated of but very little, if any, by our works, either in our journals or in our theory and practice. It is scarcely possible to find an article to-day treating on chronic catarrhal appendicitis; but, on the other hand, there is literature on suppurative perityphlitis and on the acute inflammation of the appendix. I am glad to see that my paper has done just what I intended that it should do, which was to bring out this discussion, and get an expression from the members of the society on this very intricate subject, which, as I said in my paper, is one of the most difficult to diagnose.

In reply to the first gentleman, who referred to McBurney's article: You may take McBurney's article and three or four others that were published in the *Annals of Surgery*, and you get the Transactions of the American

Medical Association and read that which appears under the head of "Appendicitis," and you will find that there isn't any of them that treat of chronic catarrhal appendicitis, but it is on some acute phase within this part of the intestinal tract. As I said in my paper, it is very easy to diagnose acute appendicitis. It is very easy to tell a man has got a whitlow on his finger, but it is not very easy to tell where it is going to go before it is ended. The remarks that I made in my paper were not intended, by any means, to have you think that they were absolute. I pointed out a few certain things—a few roads as it were, that usually led to the same town. And whilst they are not absolute, they are of a character that leads you to think, and to consider this class of diseases, and to follow them out with other symptoms that may occur to you.

In speaking of toxemia, I cannot say that the surgeon could be excused for making an operation for appendicitis, if a toxic character was apparent—a toxic condition of things. I don't see any foundation for removing the appendix under those circumstances.

Dr. Reed referred to pus-tubes. Of course, we all know that the best operators make mistakes. I saw one of the best operators we have in Philadelphia, or in this country, make a mistake. He diagnosed one thing, for which he said he was going to make an operation, but he didn't do it; he operated for what was there. In the case Dr. Reed referred to, he had symptoms enough to make an operation justifiable for chronic catarrhal appendicitis, or perityphlitis. He operated for inguinal hernia, and found a class of symptoms that warranted an operation, and the operation was justifiable.

In referring to the remarks made by Dr. Scott: We all know, as I have said, that there is no class of symptoms, that I know of, that will point absolutely and directly to a case of chronic catarrhal appendicitis in every case; and that is one of the reasons why I have brought this question up, in order that we might gather together our knowledge, and, if possible, in the years to come, formulate a line of symptoms that may enable us to more accurately diagnosticate this class of cases.

In reference to the acute phase of the disease, as others have said, I don't think it is always the best thing to do. It is a question when you should operate. I think, how-

ever, if there is any suspicion whatever that there is perforation and an escape of the contents of the vermiform appendix into the abdominal cavity, that we cannot operate too soon—that we can't get that matter out of the abdominal cavity too quick. If we have symptoms leading us to suspect anything of that character, we should operate at once; I don't know that we would be justified in waiting.

In reference to the opium treatment: I believe that the opium treatment simply confounds us. I have seen case after case of opium treatment that failed, in spite of all that we could do. I recall a case where I made a post-mortem about six months ago, in which we desired to make an operation, but the patient wouldn't let us. We found a perforation of the vermiform appendix; and not only that, there had been recurrent attacks coming on time after time, which had been treated with opium, and finally there was a complete stenosis produced at the ileo-cæcal valve, and death. The opium treatment did mislead. It misled all of us, and the final result was death.

I can only confirm what Dr. Ricketts has said, and Dr. Reed has already replied to Dr. Kinsman; I will not take up your time with that.

Dr. Hall took up the same ground which we have already gone over, opposing operations in the acute form, which I think is wise. But in referring to the recurrent attacks—you can call it recurrent appendicitis, or chronic catarrhal appendicitis—if you have a case in which there are recurrent attacks of appendicitis, let it be chronic or not, I think you are allowing an ax to hang suspended over your patient's head when you don't operate. Those cases may get well. They do get well occasionally with medical treatment; but I tell you there are more that die than get well.

And in this connection I would be very glad if this society, if they see fit, would take up this matter still further. It is one of infinite importance. We do not claim in our paper, and none of us here have claimed, that we are able to diagnosticate this disease, but we all admit that it is common. It comes to those of you who are not surgeons—it comes to you more than to those of us who make operative surgery a specialty. I would be very glad if this society would appoint a committee of five to take up this subject and report at the next meeting,

and let us have some progress. I do not think we can do a better thing than to take up the subject of chronic catarrhal appendicitis in this special manner.

NOTES ON THE TREATMENT OF SYPHILIS—ITS EVOLUTION AND PRESENT STATUS.

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The treatment of syphilis has completed a cycle, and to-day we stand upon the same ground that our medical fathers occupied nearly four hundred years ago. Familiar as they were with gonorrhœa, which even in the days of Moses was known as a "running issue out of the flesh"; familiar with the local contagious ulcers of the genitals, which had been accurately described by the most ancient medical writers of Arabia, Alexandria, Greece and Rome, they were, in 1494, amazed at the advent of a disease such as they had never seen before and whose ravages they were unable to assuage. For nearly half a century these three diseases retained their identity in medical writings, and not until the generation passed away which had observed its invasion, did confusion prevail to retard the course of future progress. Thus the earliest authentic observations in the treatment of syphilis were made while it enjoyed the distinction of being a disease *sui generis*.

It soon became known that mercury had the power of controlling the disease, and it was adopted as the standard treatment. But the indiscriminate use of this potent drug led to results even worse than those of the disease itself. It was only four years after its first appearance

in Europe that Gaspar Torella, physician to Pope Alexander VI., directed that the ointment then in use be reduced in strength, and soon after John de Vego, a Genoese, obtained brilliant results with a plaster which was a compound of mercury with aromatics.

Finally the belief obtained that these diseases, having a common origin in sexual contact, were alike in nature, which led to the abandonment of mercury as a specific. This was in the epoch of depletion, and blood-letting, starving, purging and sweating followed as a natural sequence. Nor were drugs abandoned; the New World offered numerous plants that were held in high repute among the Indians of the American continent. Foremost of these stood guaiacum, which as early as 1508 was imported into Spain, where it soon attracted attention by the wonderful cures attributed to it. Ten years later its fame extended into Italy and Germany; special treatises were devoted to it, and guaiacum remained the recognized treatment for the "venereal disease" during the sixteenth century.

Then came sarsaparilla, which in turn threatened to supersede guaiacum, and which to this day holds a high position in the popular mind as a "blood purifier" *par excellence*. But many cases, those we now know to be syphilitic, did not yield to this form of treatment, and mercury was added to the "decoction of the woods." It was in this way the decoction of Zittmann became famous, which even now is extolled by some who think it a useful adjuvant in the treatment of constitutional syphilis.

During the seventeenth and eighteenth centuries there was no consensus of medical thought, consequently there was no definite nor stable form of treatment. Some with retrospective bent adopted the "dry treatment of the Arabians," which consisted, aside from certain annoying observances, in the taking of a mercurial pill morning and evening.*

* Bumstead, Venereal Diseases, New York, 1866.

In 1758 a report was made to the Royal Academy of Sciences of Stockholm, entitled "The Lobelia, a certain Remedy against the Venereal Disease, Discovered by Peter Kalm." * In this quaint document the writer describes *in extenso* its employment by the Indians of North America, the facility with which the venereal disease was cured with it, how the secret was obtained from them, and finally a comparison in which is shown its advantages over the treatment by mercury.

With the beginning of the nineteenth century an important advancement had taken place. Benjamin Bell had eliminated gonorrhœa from syphilis in 1793, and as if to clear the way for a more definite system of therapeutical investigation which now became possible, a new system of treatment was adopted; and it became by far more general than any other since the distinct features of the different venereal diseases had been lost sight of. At this time, too, a reaction was beginning to show itself against the heroic measures for the relief of disease in general. The new method was called the "Simple Treatment," and corresponded to our own expectant treatment. As it was not known that the disease, which at this time included all venereal sores, tended to recovery, the new treatment was hailed with enthusiasm. Nor was the enthusiasm ill-timed, for it taught that the disease was self-limited; that relapses which were attributed to certain drugs, notably mercury, were a feature of the disease itself, and that much smaller doses of mercury were capable of controlling syphilis than had previously been given. †

The second great advancement was made in 1831, when Dr. Robert Williams advocated the iodide of potassium in the treatment of syphilis. The favorable report of its use by Ricord, of Paris, and the publication by Wallace, of

*Dr. Heller's Medical Notes, 1758.

†Berkeley Hill, Syphilis, London, 1881.

Dublin, in 1836 directed general attention to the drug and it soon became extensively used.

When it became known that syphilis if left to the natural bodily forces became eliminated in time, there was an attempt made to hasten the *vis medicatrix naturæ* by stimulating the surface with blisters and the application of tartar emetic ointment.

In the same direction, but by far the most important of these procedures, was that of syphilization, which was conceived by Auzias in 1846 in the following way: In endeavoring to inoculate some of the lower animals with syphilis he obtained as he supposed a saturation or tolerance of the poison whereby the syphilized enjoyed immunity from further inoculation as well as from the ill effects that were known to follow the first. Experiments made so extensively by Boeck of Sweden, and others, have relegated syphilization to the long list of abandoned methods which adorn the pages of medical history.

It was not, however, until Ricord began a systematic study of venereal sores at the Hôpital du Midi in 1850, which enabled Bassereau two years later to announce to the world the doctrine of duality, that the modern epoch in the treatment of syphilis really began. From this time it has been separated from extraneous maladies which are unlike in nature and which differ essentially as to treatment. Note the result: Uniformity of method has succeeded the indefinite age of "new systems" and "new schools"; a uniformity, too, without the loss of individual effort to attain the highest end.

Thus systematized, the primary object in the treatment of syphilis is to prevent systemic infection. With this in view the initial lesion has been cauterized after the manner of Ricord; it has been excised as practiced by Auspitz and Kolliker, but these procedures have failed to prevent the subsequent phenomena of syphilis. More recently Otis has advised the destruction, and where practical the excision of the initial lesion, not so much to prevent sys-

temic infection as to destroy a focus of infection and as the quickest way of healing the sore.

In the majority of cases the application of mercury will cause the lesions to disappear, and except when accompanied by suppuration or necrosis is the best means at our command. With suppuration, iodoform is very extensively used; aristol is a new preparation, free from the disagreeable odor of iodoform and said to be even more efficacious, while antifebrin, in the writer's experience of three months, promises to supersede all other applications in this particular condition.

But it is not with the primary sore that error is most liable to occur, for it usually disappears of its own accord; it is the systemic management, the persistent warfare against the syphilitic virus and its sequelæ that appear to be insufficiently understood by many who essay to treat the disease.

First, then, it is well to bear in mind that the virus may in time be eliminated by the natural forces of the body.

Second, the drug, and the only one, that is known to possess any curative influence over syphilis is mercury. Under various disguises it has been given for centuries, and to-day its virtues are attributed to waters, and to nostrums innumerable. But a careful analysis enables us to affirm, that under its use the various manifestations of the disease disappear, relapses are less frequent, and finally the organism becomes free from the baneful influence of the disease and may be infected again.

When to begin the mercurial treatment is a point on which opinions differ. Hutchinson* claims for mercury the power of aborting the syphilitic processes; he therefore gives it with the first appearance of the primary sore. Lassar in a personal interview advocated its early use, "because if the disease does not prove to be specific it does no harm, whereas if it be syphilitic the sooner it is given

*London Lancet.

the better for the patient." Others again withhold specific treatment until the secondary or systemic manifestations appear.

Doubtless the best course is to begin the use of mercury as soon as a diagnosis of syphilis is made. Previous to this the time may be profitably occupied by placing the patient under proper hygienic influences, and in the use of tonics such as iron, vegetable bitters and mineral acids, which will prove useful adjuvants to the subsequent course of mercury.

The special form of mercury that is advisable to use will depend largely on the individual requirements of a given case. Generally speaking the protosalts are to be preferred to the bisalts at the beginning, as calomel, mercury with chalk; or the blue pill, either alone or combined with iron as in the formula suggested by Bumstead many years ago.* In infantile syphilis mercury with chalk is largely used in England and has given the writer the best results. It was formerly thought that to obtain a therapeutic effect of mercury it was necessary to produce salivation; we know now that the best results are gained by the continued use of the drug in quantities barely sufficient to produce tenderness of the gums.

To guard against the disagreeable complications which may appear in the mouth, the following simple rules should be observed: Before beginning the mercurial treatment the teeth must receive attention at the hands of a dentist, and all decayed teeth either filled or removed. After eating they should be brushed with a soft brush to remove all particles of food, and if tenderness supervene a saturated solution of chlorate of potassium may be used as a mouth wash.

From the most remote times various methods have been employed to introduce mercury into the organism; plasters, inunctions, baths and its ingestion by the mouth, have all had their ardent advocates. Fumiga-

* Venereal Diseases, New York, 1866.

tion, which was revived by Langston Parker, of Birmingham, is an efficient way of giving it and free from some of the disagreeable features of other external methods. Of late hypodermic or intermuscular injections have been extensively used, as advised by Scarenzio, of Milan, in 1864. Lewin has especially identified himself with this form of treatment, and it has been used almost exclusively in the extensive wards of the Charitè Hospital at Berlin for the last thirty years, which of itself is a sufficient guarantee of its value and the high estimate placed upon it by this most eminent syphilographer.

But he is best able to treat syphilis who recognizes the special value of the several methods at his command. For reasons that are apparent to all, ingestion by the mouth is the one most in vogue, and when well borne there is none better. Frequently, however, gastric disturbances necessitate its discontinuance, when recourse must be had to one of the other ways of administration. The choice must be determined by the physical as well as the social status of the patient.

Inunction is very extensively used both in this country and in Europe, and forms the essential treatment at the numerous springs that have been vaunted for the cure of syphilis. The skin should be prepared for the absorption of mercury by taking a warm bath with soap, best at night. Then from half to a drachm of the strong ointment should be rubbed into the skin before the fire, either by the patient or an attendant with rubber gloves.

To those who can not subject themselves to this uncleanly process, fumigation may with equal efficacy be adopted. In fact the latter offers the best means of treatment when superficial lesions are present, as in the early syphilides. But many patients can not carry out even this, and a method less liable to exposure must be used. Here is a field for hypodermic injections, which have but one objection so far as the writer's experience goes, and that is the pain, which continues three or four

hours thereafter. Again, when it is necessary to obtain a rapid effect of mercury, hypodermic injections afford a most efficient means. The formula that has given the writer the best results is the one used at the Poliklinik for Skin and Venereal Diseases at Berlin:

℞.

Hydrarg. bichlor.	gr. ½.
Sod. chlor.	gr. ½.
Aquæ dest.	ʒ j.

M. et Sig. ʒ i. two or three times a week.

I have never seen abscesses or other inflammation follow this injection.

A fertile source of error lies in supplanting mercury by the iodine compounds. If one will take sufficient pains to investigate the treatment as carried on at the iodine springs at Hall, in Austria, one can not but be impressed at the frequency with which patients return after having been discharged free from all manifestations of the disease. This leads us to consider the rôle iodine plays in the management of syphilis.

After a certain though variable time the lesions which have been superficial and evanescent assume a more destructive and indolent form, new formations appear on the surface, others occur in the various structures within. This is known as the tertiary or stage of sequelæ. The tumors and destructive processes of this stage often endanger important organs or even life itself; these call for immediate relief. In iodine and its salts we have the best known agent for causing the rapid absorption of tissue enlargements, which, as is known, constitute the essential process in all lesions of tertiary syphilis. And clinical experience has demonstrated that these collections of granular tissue, whether in the stage of formation or decay, disappear and heal under its use. Nowhere is its action more marked than in lesions of the brain, and nowhere is it followed by more satisfactory results. In destructive processes of the upper air passages, too, its effect is almost marvelous.

The dose of the iodide of potassium, the preparation generally used, varies greatly in different persons. I recall to mind a woman at the Hospital for Skin Diseases at Blackfriars, who obtained the full benefit of the drug with a quarter of a grain, in fact a larger quantity produced iodism. Again, fifteen, thirty, sixty and even two hundred grains per diem is required to influence the disease. When large doses are given a combination of the iodide of sodium and ammonium, as recommended by Berkeley Hill, is to be preferred.

The salts of iodine are best given dissolved in a glass of milk taken immediately before meals; when taken after eating, water is the best vehicle, and it should be taken an hour after the meal.

Generally better results will follow the so-called mixed treatment; as iodine should only be given late in the course of the disease, the per-salts of mercury are selected, as in the following:

℞.

Hydrarg. perchloridi	gr. iij.
Kal. iodidi	℥i.
Aquæ	℥ij.

Mis. et Sig. Ten drops in water an hour after meals.

As soon as the special indications for iodine have disappeared it should be discontinued, and recourse had to mercury. It has been the writer's experience in difficult cases to find that the salts of iodine have almost invariably been given early in the disease. This leads to the conclusion, which is also entertained by others, that the salts of iodine are not only ineffective, but positively harmful in the early stages of syphilis.

It is also important to know how long mercury should be continued. This again will vary in different cases; exceptionally it may disappear in three months, while as many years is the allotted time even under the most systematic treatment.

After the continuous use of mercury has been withheld it is advisable to return to it at given periods for one or

two years. It has been my custom, when practicable, to give two courses of six weeks each the first year, and one course at the end of the second year.

But there are cases of syphilis which are conspicuous exceptions so far as treatment is concerned. First, we have those associated with debilitating conditions, such as tuberculosis, cancer and old age. Here the treatment must necessarily be varied, and oftentimes mercury plays an inconspicuous rôle.

There are other cases, fortunately rare, over which mercury has no specific influence. On this account grave doubts may arise as to the correctness of the diagnosis. Three cases of this kind under the writer's observation were in young, robust subjects, which yielded finally to 1–20th grain doses of the chloride of gold and sodium taken from four to six times a day.

As accessories to medical treatment the patient should abstain from the use of tobacco, and be advised as to a regular course of living best suited to promote his physical welfare. Special attention should be given to the various organs of elimination, and to facilitate their action water should be partaken of freely. A hot bath with soap should be taken every night, or a turkish bath once or twice a week. In this connection, and to emphasize the foregoing a word about the treatment of syphilis as followed at the various springs which have taken a strong hold on the popular mind, and which have become so much in vogue withal. The most celebrated establishment in Europe is doubtless that at Aix la Chapelle. It is claimed that the sulphur water at Aix stimulates the skin and other emunctories, so that a larger quantity of mercury may be given and eliminated without causing ill effects.

The principal advantage that these and other springs have over the treatment at home, arises not so much from the water, as from the care with which the smallest details of treatment are carried out.

First, the diet is regulated so that sufficient nourishing food is taken, and articles liable to cause diarrhœa, such as fruits and indigestible dishes, are withheld. The patient begins the day with a light breakfast; afterward he walks if the weather be fine, and then takes a hot bath; after which a drachm of the strong mercurial ointment is rubbed into the skin by an attendant. He remains in a warm room about two hours and drinks during this time from one to two pints of sulphur water. He then walks, dines, and walks again, partakes of a light supper, and goes to bed early. This routine is repeated daily for about six weeks, when the lesions have usually disappeared and he is allowed to return home, with instructions to return within a month or two. Amusements of various kinds are provided, so that most patients find their stay at Aix la Chapelle both pleasant and profitable.

In this country the Hot Springs of Arkansas are frequented by those who think to get the disease "boiled out" of them. But it is quite generally recognized by syphilographers that the water has no specific effect on syphilis, and in certain instances the best physicians at these springs have advised against its use.

In some of the larger hospitals of Europe, notably the Hôpital St. Louis at Paris, bathing facilities are provided for the use of syphilitic patients who are not supplied with these conveniences at their homes.

DISCUSSION.

Dr. Kinsman:

I would call attention to one point,—it is impossible to cure syphilis while the patient is suffering from malarial poisoning—in other words from the fever and ague of our region. Fournier and Leloir both relate illustrative cases.

I recall a case marked with gummatous manifestation, ulceration of throat, which steadily grew worse, until the periodic manifestations of ague and fever were recognized, when under the influence of quinine, caffein and iron he speedily recovered,—nor is this a single experience.

The best mode of antisyphilitic treatment may prove abortive from unobserved intercurrent diseases.

Caffein, in the treatment of the depressed condition of syphilitics, was I think introduced by Fournier to increase the action of other tonics.

Dr. S. S. Thorn:

I have followed out all of the processes which have been described by the gentleman this afternoon. I have rubbed in the mercury; I have given calomel; I have used injections and the fumigation treatment; and have finally resolved to open a line of practice which consists in the mixed practice *ab initio*. I insist upon tonics as an early part of the treatment. I know of no better tonic than iodide of potassium. I know it has been my experience to meet with a case of iodism; but it has been so rare that I do not count that as a factor. It is very easily cured. I look upon it as a promoter and aider of digestion. I look upon tonics as especially advantageous. From the beginning to the end, I am satisfied in my narrow field with the mixed treatment, and I believe that it is the best of all. I have punctured the skin; I have had my patients walk about the streets for hours from pain; I have had abscesses follow. I have fumigated and rubbed in until I am disgusted with all manners and forms of treatment except that which comes under the form of mixed treatment, which is one of the salts of mercury and the iodide combined. The dose is measured by the patient. You may give to one patient but a very small dose, while another patient requires a very considerable dose. That is the only way that I know, limiting the dose by the effect that it produces.

Dr. Corlett:

From time to time I have been impressed with the thought that of the diseases that fall under the observation of the consultant, syphilis is the most ill treated. This is largely due to the fact that so many physicians fail to realize that mercury alone has eliminative influence on the syphilitic poison. It is true that the salts of iodine will cause the nodular accumulations to disappear; it is likewise true that under its influence ulcerative processes heal, but these are the results of the syphilitic poison, or more properly the sequelæ; in the same way that stricture of the urethra is the result or sequela of a preceding inflammation in this canal. The discussion here has

shown that the iodine salts are given from the onset of the disease, which is contrary to the teaching of the best known syphilographers of the day. While this prevails syphilis will continue to be a loathsome pestilence from whose ravages there is no hope; treated understandingly there is no disease of equal importance so completely under the control of the physician.

RADICAL CURE OF HERNIA BY THE USE OF THE BURIED ANTISEPTIC ANIMAL SUTURE, WITH A REPORT OF THREE CASES.

BY F. C. LARIMORE, M. D., MT. VERNON.

The object of this paper is to place on record three operations for the radical cure of hernia by the buried antiseptic animal suture, performed by myself in 1890; to speak of some of the difficulties we encounter in the country, and to provoke a discussion on some of the methods of operating, that will enable us to decide which is the best. Nothing in surgery has engaged my attention with greater interest than the subject of hernia; and my highest ambition has been to bring to this class of suffering humanity the best method, knowledge and skill for its relief.

CASE I.

Robinson Bell, Morgan Center, O.; strangulated right inguinal hernia, five days' duration; patient of Dr. J. F. Shroutz, Martinsburg, O.

Case came under his observation forty-eight hours prior to his sending for me in consultation. A stout, robust farmer, aged fifty-nine, while catching sheep and throwing them over a pen, was seized with pain in right groin. He suffered three days, constantly growing worse, before he called his family physician. The doctor found him with pain in stomach and bowels, vomiting, and a lump in right groin which he diagnosed as a hernia. Tried taxis; prescribed opium internally and hot applications to the hernia.

Dr. Allen, of Utica, was called on the morning of the fifth day. The patient was placed under the influence of chloroform, and taxis employed, again with failure to reduce the hernia.

I was called in the afternoon of May 4, 1890, and found Mr. Bell with pulse, 120; temperature, 100°; stercoraceous vomiting; tongue red and dry. With the doctors present, and Dr. Leverett S. Kelsy, of Richmond, Ind., we proceeded at once to an operation.

The incision was made parallel with Poupart's ligament; the sac laid open. The stricture was very tight and the probe-pointed bistoury was inserted with great difficulty. Contents omentum and knuckle of intestine; highly congested, but both returned into the abdominal cavity. The sac was dissected loose and tucked up in the abdomen. The walls of the ring closed with four interrupted catgut sutures, the integument closed in the same way, drainage-tube in lower angle of wound, iodoform powder, sublimate gauze, borated cotton and bandage. Irrigation throughout with sublimate solution 1:2000. In four days drainage-tube removed, wound healed, no pus.

June 5, 1891. Mr. Bell says he remained in bed two weeks after the operation, when he got up, wearing an elastic truss for six weeks. He got tired of the truss and laid it aside.

It is now thirteen months since the operation. He has had no trouble, save a few weeks ago he felt a pain while washing sheep, lower down than the seat of hernia, which lasted but a short time. At this examination, I find the supra-pubic cushion of fat absent to such an extent as to create the impression that he has a hernia on the opposite side. The cicatrix is smooth, not retracted nor puckered, but can be felt to extend deep into the internal ring. No opening can be felt by the side of the spermatic cord. Standing and coughing fail to protrude anything, or make any impulse on the finger.

CASE II.

Patrick Keenan, aged forty-two, Chesterville, O., consulted me at my office, September 15, 1890. He stated that when a boy he was ruptured and wore a truss. April, 1890, he was struck on the abdomen with a cant-hook. A lump came down in the right groin which went back. In August it came down again, and he could not get it back. It kept so painful and swollen he could not wear a truss.

I found a large, oblong swelling extending from the right inguinal ring, deep into the scrotum, not tympanitic, not translucent, and indistinctly doughy. I put in a hypodermic needle and removed a syringeful of clear fluid; diagnosis uncertain. Hydrocele or hernia, or both. Advised an operation. Right irreducible scrotal hernia. Operated upon by Dr. J. W. Williams, of Chesterville, O., and myself, September 19, 1890. Incision over long axis of tumor. Sac laid open, contained water. A large mass of omentum adherent to sac and its folds with each other. The omentum was ligated off. The deep structures sewed up with a double continuous catgut suture, drainage-tube, interrupted skin sutures. Iodoform powder, sublimate gauze and bandage.

During etherization, Mr. Keenan thought he was in hell, and was restrained with difficulty. Ether did not bring him down, but when it was nearly exhausted, a few inhalations of chloroform completely subdued him. Two of the assistants fainted. The catgut was poor, and much time was lost in ligating the pedicle, silk finally being used for this purpose. In one week the dressing was changed, and drainage-tube removed. The wound had not entirely healed.

November 24 I saw him again. He had been in bed in the meantime with suppuration of the wound. Removed quite a mass of ligature material; from now on the wound healed, and after eight weeks in bed he got on his feet, wearing an elastic truss.

June 6, 1891, Mr. Keenan is still wearing his truss. There is an opening which admits tip of index finger. On standing, bowel protrudes. Radical operation a failure. The wound was infected at the time of operation. I present herewith the omentum removed, twelve inches square, with strong bands of adhesion between its folds.

CASE III.

Alexander Van Winkle, widower, aged sixty-four, Rossville, O., strangulated left inguinal hernia. Has had hernia for many years, and had not worn his truss for ten years past.

Drs. S. R. Stofer and T. E. Jefferson, of Danville, O., were called several hours after strangulation occurred. They applied the usual remedies, including taxis under anesthesia with chloroform, and failed to reduce the hernia.

I was summoned at midnight by telegram. The tumor was fist size. Pulse, 100; temperature, normal; with occasional vomiting of a green liquid. As soon as daylight came, we operated, after trying taxis. The patient was a first-class subject for an operation, being somewhat emaciated and with good muscular development. The sac was quickly reached in the usual way, and found to contain almost eight inches of small intestine and a mass of adherent omentum. The intestine was drawn down, liberated and replaced in the abdomen. The omentum was ligated with tendon and cut off. The sac, old and thickened, dissected up into the internal ring, well drawn down and sewed evenly across with a double continuous suture, and tied. This was excised and the stump replaced within the ring. The deeper parts were closed with a deep layer of double continuous tendon sutures, beginning at the inner angle. When the outer angle was reached, a second layer of sutures was continued back in the same manner, approximating the structures external to the pillars of the ring. The skin was approximated subcutaneously by a blind single continuous

stitch, inserted with a Hagedorn needle. No drainage. Iodoform powder dusted on the wound. Sublimate gauze, borated cotton and bandage.

It was discovered in sewing up the hernial opening, that the testicle had been included in tearing up the sac and the spermatic cord had been ligated off. So the opening was closed up complete.

November 3, changed the dressing; found the wound completely healed. No œdema of the scrotal tissues, and no pain or general disturbance of any kind. The tendon used was from the tail of the kangaroo. Every stitch was concealed.

Removal of the testicle, while accidental, was the proper thing to do, as it proved to be cystic, and enabled me to close up the opening so completely, without fear of subsequent compression of the spermatic cord and its results. The highly satisfactory result in this case has suggested to my mind, that removal of the testicle should constitute a legitimate part of the operation in most of the cases.

The sac, testicle, and the excised omentum are hereby shown for your inspection.

Dr. S. A. Stofer writes me, under date of June 9, 1891, at Danville, O., that Mr. Van Winkle still wears his truss, but says he does not need it, and that he is just as sound as when a boy, as far as hernia is concerned. There is no protrusion or impulse when he coughs. The case is a complete success.

Two of the cases reported were strangulated hernia, and of several days' standing, both in aged persons, with their lives in imminent peril. The summons was hasty, and afforded no time for study or preparation, demanding that a surgeon who would conscientiously discharge his duty toward such patients, must work them out in advance, and carry in his mind plan and details of the operation.

Since my graduation in 1869, it has become the practice to combine the radical cure with the operation for

strangulated hernia. In the study of this subject, I have been surprised by the number of methods recommended, each backed by an enthusiastic surgeon, with good results to show for the faith he has in his operation. Six different operations from which we can select, are as follows: Barker's, Ball's, Banks's, Macewen's, McBurney's and Marcy's.

Barker's method consists in exposing and freeing the neck of the sac in the inguinal canal. Opening it some little distance to the distal side of the upper end of the canal, seeing that all the contents are reduced, tying it off to the proximal side of the opening made in it, and dividing it below the ligature, using the stump to occlude the internal ring. The walls of the canal are brought together by interrupted sutures one-fourth of an inch apart, passed from within outwards, care being taken not to include the spermatic duct. These sutures, when tied, are cut short and allowed to remain. The skin and superficial fascia are brought together by interrupted sutures.* He reports thirty-five cases of hernia operation, done at the University College Hospital, under antiseptic precaution, with rapid recovery.

Ball's method consists in exposing and isolating the sac (opening the sac if necessary to complete the reduction), grasping into the neck after the contents have been reduced with the broad catch forceps, and gradually twisting it until it is quite tight. Then a stout catgut ligature is passed around the twisted sac as high up as possible, tied tightly, and the ends cut off short. The next step is to pass two sutures of aseptic silk through the skin at a distance of about an inch from the outer margin of the wound, through the outer pillar of the ring and the twisted sac in front of the catgut suture; then through the inner pillar of the ring and skin upon the inside. The sac is now cut off in front of the sutures, they preventing

* *British Medical Journal*, London, Dec. 3, 1887, p. 1204.

it from untwisting. He * reports twenty-two cases of his own, besides a number under his observation done by other surgeons. All recovered.

Banks's method consists in exposing and freeing the entire sac, opening it to make sure that all its contents are reduced. The sac is then pulled well down, tied up as high in the canal as possible before removing it. The pillars of the ring are brought together by two or three silver wire sutures which are left in position. In August, 1887, at the meeting of the British Medical Association, Mr. Banks reported a tabulated list of 106 cases, with an analysis. † Sixty-eight cases were without strangulation; in thirty-eight strangulation was present. In the analysis of his tables, Mr. Banks states, "that in the sixty-six cases which he has been able to follow up, forty-four were completely successful from a curative point of view."

Macewen's operation consists in exposing and freeing the entire sac, when with the contents reduced, the sac not having been opened, it is puckered up, and reposited in the abdomen. The manner of closing the canal by Macewen is quite ingenious. The conjoined tendon is penetrated in two places by the threaded hernia needle—first from without inwards, near the lower border of the tendon, and second, from within outwards as high as possible on the inner aspect of the canal. One single thread is withdrawn from the point of the needle, and the needle, with the other extremity of the thread, is removed, the loop presenting upon the abdominal aspect of the tendon. That portion of the stitch which comes from the lower border of the tendon is threaded by a second needle, and guided by the index finger, placed in the inguinal canal, is introduced from within outward on a level with the lower stitch in the tendon through Poupart's ligament and the aponeurotic structures of the transversalis, internal and external oblique muscles. The needle

* *British Medical Journal*, December 10, 1878, p. 1272.

† *British Medical Journal*, August 10, 1887, pp. 1259-61.

is now freed from the thread and withdrawn, and again threaded with the suture which protrudes from the upper border of the tendon, when it is introduced from within outwards on a level with the upper stitch in the tendon, through the transversalis, the external and internal oblique muscles. It is then freed from the thread and withdrawn. The same stitch may be introduced lower down the canal if thought desirable, when the ends of the respective sutures are drawn tightly together and tied. Macewen also brings together the pillars of the external ring. Mr. Macewen tabulates eighty-one cases without a death, and with a firm occlusion obtained before leaving the ward.

McBurney's method consists in exposing the sac throughout, when it is securely tied with a stout catgut or silk ligature at the internal ring, the sac being always opened to demonstrate that no portion of its contents remains unreduced, also to enable the operator to insert one or two fingers through the neck and a little way into the peritoneal cavity. This is done to positively guard against the return of a piece of the intestine or omentum into the sac during the application of the ligature. The ligature is tightened on the operator's finger and tied very tightly as the finger is withdrawn, leaving a pedicle of sufficient length to prevent slipping of the ligature. The rest of the sac is cut away. In preparing the wound for packing, from four to eight stout silk or catgut sutures (according to the size of the wound) are passed through the tissues which form the upper wall of the wound; namely, the conjoined tendon, the aponeurosis of the external oblique, including the inner pillar of the external ring, and the superficial fascia and skin, the skin being carefully and deeply inverted while each stitch is tied tightly. The same number of sutures is passed through the tissues forming the lower wall of the canal, namely, the skin, the superficial fascia and Poupart's ligament, including at the lower part, the outer pillar of the

external ring; these are also tied tightly, exercising the same care to invert the skin. The object of these sutures is to positively secure patency of the wound for the formation of granulations from the bottom. Next, to lessen the width of the wound, two or more heavy stitches are carried across it, passed deeply through the skin and superficial fascia, these latter relieving the tension on the sutures so that they do not cut their way out too soon. The two last or tension sutures are tied over pledgets of iodoform gauze to avoid ulceration. The wound is now carefully irrigated, dusted with iodoform throughout, and firmly packed with iodoform gauze. The portion of the wound beyond the canal is sewed up without packing, catgut or other drainage being inserted at the lower end if required. The packing of the wound after having lessened its width by the introduction of the tension sutures, secures firm union, both from below upwards and from above downwards.* By this method McBurney has now operated more than forty times with only one death--which had no connection in any way with operative procedures—and with only one return of hernia.

Marcy's operation is essentially as follows: The hernia sac is opened, its contents restored to the abdomen, or removed, and it is freely dissected to its very base within the ring. The sac is then drawn down, sewed across with an even double suture, resected and removed. The peritoneum is then carried quite within the ring. In large direct inguinal hernia, it is important to restore the obliquity of the canal: nature's wise provision for maintaining the closure of the canal by intra-abdominal pressure, in health. This is effected by commencing at the lower and inner border of the ring, the cord having been laid to one side, and closing by double suturing, quite to the inner opening of the ring, upon the cord, which is now re-

* *Journal American Medical Association*, November 2, 1889. Remarks by T. H. Manly, of New York City.

placed; and the external pillars of the ring are closed by similar sutures downward and inward to the pubic tubercle. The inguinal rings are thus reformed, and the canal is restored to its normal oblique position. If the tissues are sufficiently thick to warrant it a further layer of animal suturing completes the closure, and the skin is neatly and accurately approximated by a blind running stitch, perhaps best applied with a fine Hagedorn needle, taken through from side to side, including only the deeper layers of the skin. This completes the closure of the wound, each step of which is done under irrigation with a weak sublimate solution, and without the vestige of a stitch in sight. The incision is now dried, dusted with iodoform and covered with iodoform collodion, into which a few fibres of cotton are incorporated. No further dressing is necessary. And in no instance does he advise the wearing of a truss.

These are all cutting operations, and are sometimes known as the open wound method. The scholarly genius and indefatigable industry of Dr. Henry O. Marcy, of Boston, has impressed me so much that I confess they have biased me in favor of his method of operating for the radical cure of hernia. While I profess to be a disciple of Dr. Marcy, you will see, in the report of cases, how poorly I have imitated him. Indeed, in Case I the deviation has been so great as to make me doubt myself about classifying it as a Marcy operation. In none have all the details been carried out; the third case, however, approximating it more nearly than the others. It is my determination to make the fourth case, should it present, a Marcy's case, pure and simple. I am gratified with the result in my own cases. The method was not to blame for the case of failure.

In a letter to me, dated Boston, May 29, 1891, Dr. Marcy says: "My later experiences cause me more distinctly to emphasize my views, and in all of the large list of operations which I have performed I can assuredly say

that not a single patient has apparently approached the danger line where the abdominal organs were not themselves involved by constriction through the hernial openings. So far as I am enabled to determine about 90 per cent. of the cases remain cured. I am quite sure that the tendon suture is here greatly to be preferred to catgut, and should supplant it as a suture material in the entire realm of surgery."

The tendon of the tail of the kangaroo is by far the best, specimens of which, by Dr. Marcy's courtesy, I can exhibit to you. These six tendons vary from eighteen to twenty-four inches in length. They are very strong, and cannot be broken with any ordinary strength in tying. They seem large, but are the size preferred for hernia. The tendons are ready for chromicising. The single, darker one, is as it appears when ready for use. Australia is their habitat. They cost from five to ten cents a tendon.

The operation is simple and easy, with no attempt to dissect out, still less to see all the anatomical coverings of a hernia, to avoid wounding the epigastric artery, or finding the obturator from the epigastric skirting the free margin of Gimbernat's ligament. How to insert the double continuous suture is a little difficult, at first, but can be mastered by attention to the description and cuts and after a little practice it becomes as easy as the old method of suturing. In my operations I did not have a good needle, using an ordinary perineum needle, eye near point, and in handle. To-day I received from Codman & Shurtleff, of Boston, Dr. Marcy's improved needle, which is shown you. The large curve, large and peculiar-shaped eye—cutting, shear, round point and very smooth polish, will all make the operation easier and save time. The needle should be used in sewing off the sac, since it closes in even double seam.

In the third case I was completely astonished how neatly and accurately the skin was approximated by the

blind, continuous suture. *No* gauze, *no* cotton, *no* bandage and *no truss*, will certain strike every one as laying aside many restraints and showing sublime faith in the result of the operation. Sublimate solution, 1-2,000, was used throughout, and although no symptoms of sublimate poisoning were manifested in either case, I shall take the precaution, in all my future operations, to use sterilized water only for irrigating instruments and hands after commencing the operation.

This conclusion is reached from a statement recently made to me by Dr. Hunter Robb, in Johns Hopkins Hospital, Baltimore, who has operated on from twenty to twenty-five dogs. "He would first irrigate the abdominal cavity, through an incision about three cm. long, *for two minutes*, with 1 to 20,000 corrosive sublimate solution, and then immediately afterwards irrigate with sterilized water at the same temperature, 35° to 37° C., for the same length of time, and then sponging out with a sterilized sponge, making the cavity as dry as possible."

In every one of these experiments the dogs died in from twelve hours to five days, and without exception he found the well-marked lesions of corrosive sublimate poison.

So convinced are they of the dangers of corrosive sublimate in the peritoneal cavity, *that they never use it, in any strength, with their operations on the pelvis*, and, in fact, they very rarely, if ever, use it with douches; and in one instance enough of the douche was retained to produce mild bichloride poisoning.

Dr. Robb is still working on this line of experiment, and will publish his results when finished. In this, as in all other operations, I practice strict asepsis throughout. Two good and reliable assistants are indispensable. In strangulated hernia lose no time, but proceed at once to afford relief with an operation.

My own feeling, in the conclusion of this paper, cannot be better expressed than in the noble words of Sir Spencer

Wells, who wrote in 1858: "The relief of a strangulated hernia is justly regarded as one of the noblest triumphs of operative surgery. The surgeon saves the life of the patient without removing or deforming any part of his body. But the surgeon who cures hernia radically, with certainty and safety, is a greater public benefactor, as he not only relieves large numbers of his fellow-creatures from suffering, but he averts the danger of a strangulation, to which they are continually exposed, in a greater or less degree, through every period of life."

DISCUSSION.

Dr. R. H. Reed:

Of course, the great trouble with the radical cure of hernia has been to make such an operation as would be radical. It is not difficult to make the operation. Many of us have undoubtedly made the operation over and over again; but the great difficulty has been in past years to get an operation where the hernia will not return—in other words, where the hernia will stay cured when you have operated. There is no question, in my mind, but that either the Marcy operation, or some modification, is the proper operation for the radical cure of hernia. The closing up of the opening in the way designated by Dr. Marcy is the important point in the operation. I have made a few operations for strangulated hernia, and for the radical cure of hernia, making the operation for ten or twelve years past—I think it is, since I have adopted it—not exactly Dr. Marcy's operation, because what I have adopted was prior to my knowledge of his method, but in the same line; and I don't know but what the very next operation I do I will adopt his method entire, because I admire Dr. Marcy as well as his operation; and that is to close that opening completely by dissecting out the sac, and bringing it out as far as possible, and instead of sewing it, I have ligated it, and have included it in the sutures, in suturing up the pillars. I have done that in all my recent operations. I have no knowledge of any ever returning since I have adopted that plan. Before I did that, I used to push the sac back into the abdominal cavity, and close up the wound, and tie the various sutures through the ring, and then I have had the hernia return; but since I have adopted the other plan, I am satis-

fied that it is much safer than any I have seen yet. Dr. Marcy's is a great improvement on anything I know of.

Dr. W. E. Wirt:

I have had the pleasure of assisting Dr. Bull, of New York City, in the Hospital for Ruptured and Crippled. As he had charge of the hernia department, I saw him every day, and had talks with him from time to time about his operation and his opinion of the radical cure of hernia. He keeps a very complete record of every case that comes in there. That shows the entire history of the case, and if there has ever been an operation. Their names are taken, the blanks filled out; who operated, and how long since the operation; how long they had it before the operation—a complete history. In that way he finds out all about a case, before, and during an operation, and after the operation. He told me in regard to the subject that at a meeting at Albany of the New York State Medical Society he had read a paper on the subject. In this article he cited three or four hundred cases—a large number, and I think that was the number—and that he took no case under six years, because, he said, that you might for three years report a case as a radical cure, and in the fourth year it might prove to be a recurrent case. Therefore he took no case under six years. In this paper, and with perfect statistics on the subject, he proved that a very large per cent. of the cases recurred; so that now he calls the operation nothing but an aid to the truss. In the hernia department it was always spoken of as simply an aid to the truss. He said that one gentleman from Buffalo got up and said, "Gentlemen, I was about to make some remarks on this subject, but the number of my cases is so much smaller than that of Dr. Bull, and his statistics are so complete, that he has taken the wind out of my sails, and I have nothing to say." He shows up Dr. McBurney's operation. We had a number of those cases come in there, and I have seen them. He does not oppose the operation himself, but he says it is merely an aid to the truss. I remember one very distinctly, because every one in the hospital, internes and all, was called in to see it. It was a very large scar—stretched that much. I think it was three inches and a half in all directions; and it was so thin that you could see the peristaltic action of the bowels—probably as thin as tissue paper. The patient was so sensitive that if you put your

hands toward him he would jump. The doctor said the only thing left was to cut out the tissue and bring the parts together, and do what he could for the man, then it would be an aid to the truss. In all cases at the hospital there they insisted on a truss. After the tenderness is out the doctor puts on a truss, and makes the pressure very light—just so that it will sustain the parts in coughing or anything of that kind. Then the patients have to make a report every month, etc.; and in that way they keep track of them as long as they can. I asked the doctor what kind of cases he would recommend for an operation. He said there are two kinds of cases that he would especially recommend; one is where a man is vigorous and rugged, and would likely stand the operation perfectly well, and there would be no danger in the operation; where the rupture is so located that there would be no strain, and it is a great deal of trouble to the patient. Another class of cases are those which cannot be held by a truss; as in the case of very hard-working men, perhaps, who could not work under those circumstances, then they would need an assistance to the truss, and that assistance is his operation. By the operation the wall is strengthened; and although you cannot promise a cure to the man, or say to him that it will never recur, yet the truss will exert a certain amount of pressure, and in that way it can be held in place so that the man can attend to his business. Those are the two kinds of cases.

Dr. Larimore:

In closing the discussion, I want to bring to your minds more emphatically than I did the great stress which Dr. Marcy lays on the preservation of the obliquity of the inguinal canal. He attaches greater importance to that than perhaps to any other one thing. The deeper parts are stitched, and then the return row of stitches is more superficial; and in that way the obliquity of the canal is restored. You all understand that the internal ring is about an inch and a half wide, a little bit outward from the external ring. I shall attach greater importance to that, since the result of Dr. McBurney's operation was told by Dr. Wirt. I have had no experience with McBurney's operation, but I have yet to go back on Marcy's. That is the one I have studied. Another thing I wish to speak about is the tendon of the kangaroo. This aseptic animal suture is put in, and it does great work. The

tendon excites the deposition of connective cells, and a new connective tissue structure is organized there and takes the place of the suture, the suture being gradually absorbed. Dr. Marcy claims no originality in that matter. He first got this idea from Lister in '69, when Lister began his ligation of arteries with catgut. All the originality he claims is having used it in other tissues. In all operations over the body he now uses the kangaroo suture. It does seem to me that we can rely on this operation as being definitely and permanently curative; the obliquity of the canal having been restored, the new connective tissue taking the place of the sutures, all the deeper parts being solidly and completely grown together, making the man a better man than when he was first constructed. Dr. Marcy has been performing this operation since '69—twenty-two years. I don't know that I can go back to his earlier cases. I certainly should be very much disappointed if they are not permanent.

A PLEA FOR A MORE EXTENDED SUPERVISION OF THE PARTURIENT WOMAN.

BY D. R. SILVER, M. D., SIDNEY.

In at least one-half of the cases of obstetrics which physicians in the country and smaller towns are called upon to attend, responsibility for their proper management is coincident with the beginning of labor, and ends twenty-four hours after delivery. It is the practice of some to make no second visit. When an engagement is made by the prospective father, or other interested relative, it is not for the purpose of obtaining information or knowledge, but to secure service at a certain time; and the part taken by the doctor in this interview is to enter the item in his note-book. The matter then passes from his mind, only to be recalled, perhaps, by the arrival of an excited and impatient messenger, urging immediate attendance at the bedside of the woman, who, some days, or perhaps two or three weeks, before the expected time for labor, has fallen in a fit. Then there is a time of hasty preparation on the part of the doctor. Whether a beginner or a veteran he knows what puerperal eclampsia means, and he trembles to think of the probable outcome of his case. In the consultation which is almost sure to follow this hasty summons, everything is talked of except the one important matter, namely, how might this great calamity have been averted? It is not the province of this writer to discuss that question. It is taken as granted that much might have been done, but the golden opportunity was lost when the engagement was made, if, indeed, it had not passed before. The answer to a single

question propounded by the vigilant physician is often sufficient to show whether all is right or whether something is wrong. And, unfortunately, the laity assume that every pregnant woman has certain symptoms, which, while unpleasant, are not dangerous, and call for no treatment. Thus it is that in scores of instances the first intimation the physician has that pregnancy exists is when he is called to treat an eclamptic seizure.

Prevention is better than cure, and it is scarcely questionable longer that proper management may avert a seizure which, untreated, might at its first onset produce fatal injury to the poisoned and oppressed brain. We know this, but the people, our patrons, do not know it. Who then is responsible? Manifestly the profession has been, and is now, derelict in the performance of duty, by failure to inform the people that medical science is able, at least in some cases, to avert that fearful calamity, an attack of *puerperal convulsions.*

From the physician's standpoint every pregnant woman should be regarded as in a pathological condition, or at least subject to certain maladies pertaining solely to that condition. Women should be taught to feel that for nine months before confinement, and certainly for as many weeks afterwards, they need the counsel and advice of a skillful, conscientious physician. When that happy day arrives, and it is sure to come, we shall have fewer broken-down and bedridden mothers, fewer cases of one-child sterility, and far less use for the abdominal surgeon.

It is true that women who have not borne children are subject to displacements of the uterus, ovarian tumors, and the various kinds of pelvic diseases—salpingitis, ovaritis, metritis, etc.—still, in the great majority of instances such cases occur in those who have borne children, and can be traced to pregnancy and labor as causative factor. It were strange indeed if a process so complicated as that of labor, so straining upon the nerves and muscles, when both the mental and physical systems are wrought up to

the highest pitch of excitement, should not be full of dangerous possibilities. Obstetric labor, the bringing of a human being into the world, endangers the integrity of parts of the physical system necessary to the continued health and comfort of the mother. Serious injuries are by no means uncommon. And yet, at the conclusion of labor, no one scarcely attempts to find other lesion than that perhaps of a lacerated perineum. Indeed, no one, however skillful, can determine at this time whether the parts will or will not assume their normal relations, position and condition. But we know that unless involution is perfect, not only of the uterus, but of every organ concerned in reproduction, good health for the unfortunate is impossible. That proposition is indisputable. And yet in obstetric practice responsibility ceases, and oversight and care cease for the physician, at the second visit, twenty-four hours after delivery. The result is just what we find, that a large percentage of mothers are invalids, and through no fault of their own, either.

Nine days in bed is well; nineteen were better. But rest nine months will not compensate for the lack of professional skill in restoring a displaced uterus or ovary, stitching up a lacerated cervix or perineum, or properly treating a tender and inflamed peritoneum injured in the throes of labor. At our second visit we cannot ascertain whether the uterus will or will not assume its proper position in the pelvis. The chances are against rather than in favor of it, and therefore, when a sufficient time has elapsed, say from nine to fifteen days, a third visit should be made, when it will be possible to make out the state of the involution, the position of the uterus, and the condition of the ovaries and pelvic organs.

No woman having intra-pelvic disease should be allowed to pursue her ordinary vocations. No woman having such disease gets well without rest and treatment, and especially in case of displacement she must have the counsel and skillful personal care of the physician. Sev-

enty-five per cent of the cases of inflammatory pelvic disease which I have seen could be traced to labor or confinement as the predisposing cause, and I am quite well convinced that at least one-half of these might have been averted by taking such precautions as I have before indicated. Backward displacements of the uterus are especially liable to occur after confinement. The text-books speak of these, and mention tight bandaging as a cause. Of course, it is possible that an obstetric binder might be placed so that it would cause a retroversion; but, in fact, whether a binder be used or not, the enlarged heavy organ is very likely to remain in the position in which it is left by the attendants; all the parts are relaxed; the fundus, even if properly placed, is liable to fall into the pelvic excavation, becoming retroverted, and will remain so. There is, indeed, nothing to pull it into position except the round ligaments, and their power is very questionable indeed.

Every woman arising from her bed of confinement with a displacement, or a pelvic inflammation, is a candidate for chronic invalidism—the cases on which quackery thrives. The lesion, easily amenable to treatment at its beginning, soon brings changes which cannot be remedied even by the skill of the most expert gynecologist. There is then a life of misery before the unfortunate patient.

The question for us to seriously consider is, who is responsible, and what can we do to prevent the present large percentage of mortality from puerperal eclampsia? I would answer, that we are responsible, not our patrons. How should they know the danger signals?

Of course, I know there is and should be a certain delicacy in giving unasked advice. But I take it that we should educate our people, and that every mother and every prospective one should be informed of the importance of receiving counsel and advice in the early months of pregnancy. By concert of effort by the profession in this direction the percentage of mortality could be largely cut

down, and pregnancy robbed of one of its greatest terrors.

The fear of maternity, of the pains of labor and of the results known so often to follow delivery, hangs like a pall over the lives of expectant mothers. This dread leads to the practice of all sorts of expedients to avoid motherhood; not because children are not wanted, but because pregnancy is feared; not because they love children less, but because they prize life and comfort more. Thus it is that society, the nation and the world suffer. It is true that with anæsthesia we have done much to lessen the dread which every sensitive soul has for the travail of childbirth. But observation teaches me that the mental suffering consequent upon the knowledge of the possible danger to life far outweighs the fear of physical suffering, and that with both in prospect it is not wonderful that motherhood is avoided when possible, or only accepted as a religious duty.

The time has come when there is a demand for better obstetric service. Among the more intelligent and wealthy the day of the midwife is past. There is demand for skill, a demand for men with clean hands and pure lives, and we are beginning to feel as never before the responsibility of this demand. Among the younger men it is not unusual to hear this assertion: "If it were not for my family practice I would eschew obstetrics. It does not pay. Too much responsibility." To the scientist this is encouraging. It is only when men feel responsibility that duty will be well done.

In conclusion, I would say: There is need of reform in obstetric practice. Its scope and limits have been before indicated. The time has fully come when there should be an advance all along the line, to the end that the price of motherhood may be less than possible death or chronic invalidism. To accomplish this end it is not alone sufficient to give better training to the young men just entering the profession, that they may skillfully use

the forceps, deliver a retained placenta, control a hemorrhage, or restore a lacerated perineum; but it is necessary for all of us to recognize the fact that no amount of skill can avoid or prevent accidents whose consequences can only be seen long after the patient has passed out of our care, as is now the fashion on the third day after confinement. Every gynecologist knows the large part played by displacement of the uterus in the complaints of sick women. He knows too well, also, the great difficulty experienced in its restoration, and that the changes wrought by gestation afford the best hope for cure. But confinement will accomplish nothing unless the organ is held in place by mechanical support, if necessary, until involution is accomplished and all the pelvic viscera have assumed their normal relations and positions. Impregnation of the displaced and diseased uterus does not often occur, and we can number these as causes of one-child sterility which is now so frequently observed. I would then plead that our care of the parturient woman should commence with the beginning of pregnancy, and only cease when we are fully satisfied by careful investigation that every organ concerned in the reproductive act is in its normal healthful condition.

CASES OF COMPOUND GANGLION.

BY CHARLES S. HAMILTON, M. D., COLUMBUS.

The writer of the paper has thought it worth while to report these cases, for the reason that they illustrate one method of successfully treating an affection which is otherwise obstinate, while at the same time they throw some light pathologically upon its structure. By way of preface, a few words may be said as to the pathological anatomy of that which we call a ganglion. The simple ganglion is, briefly, a small cyst in the course of, and more or less intimately connected with, the sheath of a tendon. Its precise mode of origin is unknown. From our present knowledge we cannot say certainly whether it be a neoplasm, a tumor, in other words, or the result of a chronic teno-synovitis; though the probabilities are in favor of the latter hypothesis. The fluid of the cyst varies greatly in color and consistency, and frequently contains the well-known melon-seed, or fibrinous bodies. Simple ganglia occur commonly about the extensor tendons of the hand, and the principles of treatment are too well known to call for comment here.

The compound ganglion is not a compound cyst, as its name implies. It is an enlargement, probably from inflammation of several tendon sheaths adjacent to one another, and is found especially on the flexor aspect of the wrist and hand. It consists of a fluctuating swelling in the neighborhood of the annular ligament, in which, by firm palpation, individual tendons, much enlarged, can be detected. The cases which are reported below are examples of this disease on the extensor side of the upper

limb—a site at which their occurrence is comparatively infrequent. There is some reason to believe that both classes of ganglia are tubercular in their nature, but as yet this point is unsettled. For the treatment of compound ganglia we are advised by writers on surgery to use iodine externally, blisters or pressure, with fixation and support of the affected member; and the writer's experience leads him to think that such treatment will effect a cure, if applied when the trouble is incipient. In advanced cases, however, it will be found that more radical procedures are necessary to ensure relief. Hence our authors recommend, in case simple remedies fail, puncture of each sheath with evacuation of melon-seed bodies, if there be any; or puncture, and the injection of iodine, with the utmost caution to avoid the introduction of septic material, which might cause extensive cellulitis and destruction of tendons in the forearm. Finally, since the day of antiseptic surgery, it has been found that the sheaths might be laid open and scraped, or subjected to any reasonable surgical manipulation, provided extreme care in regard to cleanliness was observed.

CASE I. *Compound Ganglion, Extensor Aspect of Left Wrist—Cure without Operation.*

W. G., Plain City, O., aged 15, had sustained a fracture of the ulna and a severe sprain of the wrist three months before coming under the writer's observation. He then had a soft, semi-fluctuant swelling on the back of the wrist below the annular ligament. Several enlarged tendons could be felt in the swelling. The hand was weakened though use of it was not painful. Under the direction of his father, an able physician, iodine and belladonna were applied regularly, and a leather splint which fitted snugly about the wrist was worn for a period of two months. By that time the hand regained its former strength and the swelling was gone.

This case illustrates the value of simple remedies when used early; for the disease was comparatively slight.

CASE II. *Compound Ganglion, Extensor Aspect of Both Wrists—Operation and Cure.*

A. W., Rendville, O., age 12, was admitted to Mt. Carmel Hospital, in May, 1890. Eighteen months before she began to have trouble with her wrists, and noticed some swelling on the back of each hand. She had no pain except while using her hands, which are decidedly weakened. Below each annular ligament is a soft swelling in which extensor tendons in distended sheaths may be felt. As she had been treated without benefit at her home, operation was advised and performed—first upon the left hand. On the night previous the hand was thoroughly scrubbed and disinfected, and a weak carbolic acid dressing left on until morning. An incision was made through the length of the synovial membrane which surrounds the extensor tendons, and clear fluid with some melon-seed bodies escaped. The interior of the sac was red, vascular and roughened, hence the curette was applied until its walls were smooth. The distended sheaths of the several tendons were then freely opened, and found to contain a granular material with some fluid. The tendons were apparently eroded by a destructive or ulcerative process. The granular matter was next scraped away cleanly, and the various sheaths closed by fine catgut suture. The synovial membrane was stitched with gut, the skin with silk—a few strands of gut having been inserted for drainage. An antiseptic dressing was applied and the hand put upon a splint. The dressing was not removed until two weeks later, when the wound was completely united. In September a similar operation was performed on the right hand. When the patient was discharged there was no trace of the disease and the use of her hands was entirely restored.

CASE III. *Compound Ganglion of Left Hand—Operation and Cure.*

Mrs. J. N., Delaware, O., had had a compound ganglion on dorsum of left hand for two years. It involved the tendons of the index, middle and ring fingers. In January

of the present year she was admitted to Mt. Carmel Hospital. A year previous the swelling had been opened and pressure applied, but within eight weeks was as large as ever. As she was a seamstress the disability was a serious thing, interfering as it did with the pursuit of her occupation. Hence an operation was performed. Clear fluid and fibrinous bodies escaped on incising the synovial sac, as in Case II, and the same steps were repeated, except that it was found necessary to open only one of the sheaths, the disease not being so extensive as in the other case reported. The patient was discharged in three weeks, and has since had no return of the trouble.

In conclusion, reference should be made to the structure of compound ganglion as seen in the cases operated upon. The three tendons of the ext. communis dig. and that of the ext. indicis pass beneath the annular ligament in a common compartment, and are there invested by a common synovial membrane. This membrane was in a state of chronic inflammation, evidenced by thickening and roughening of its walls, and hypersecretion of fluid; and being distended, it enlarged in the direction of least resistance—below the annular ligament. This constitutes one separate and distinct feature of the ganglion. Another inflammatory process is found within the sheaths of the tendons, as manifested by increase of secretion, accumulation of granular or gelatinous matter, and more or less destruction of the tendon itself. Probably the distention of the synovial membrane is secondary to disease of tendinous structure passing through it. It is a recognized fact that in a certain proportion of cases a stage is reached when pus forms, with resulting involvement and disintegration of neighboring joints.

The writer has not attempted to describe the whole pathological appearance of this disease, but rather to explain one phase of it, as seen in cases operated upon; and he has personally felt the need of clearer ideas upon the subject, owing to the great vagueness, and many conflicting points, in the descriptions found in works on surgery.

THE A. C. E. MIXTURE.

BY J. C. REEVE, M. D., DAYTON.

I have responded to a request to prepare a paper on the anæsthetic known as the A. C. E. mixture, not only in order to present my individual experience with it, but from the conviction that the merits of this mixture are not duly appreciated by the profession, and from the fact that nowhere, not even in special treatises on anæsthetics, is there to be found a full and complete account of it. Moreover, further study of the subject of artificial anæsthesia is justifiable so long as the present diversity of opinion prevails as to which is the best agent for producing it. The diversity of opinion upon this point is striking in amount and extreme in degree. In our own country, were the profession to be polled, ether would doubtless receive a considerable majority of the votes. Still, chloroform has many able advocates; men of ability, of position and of large experience, and throughout a wide section of this country, the Southern States, chloroform is the favorite anæsthetic. In Europe the case is far different. During several visits there, I have never seen ether administered. Some years ago I went to Italy, under the impression that it is the home of etherization. Visiting many hospitals of several of the leading cities, I did not see ether once used. During the same journey, I was unable to find an ether inhaler in the city of Paris. There, as in the hospitals of several cities of Germany, visited last year, I saw only chloroform administered. In England, ether is gaining ground. A reaction against chloroform began soon after the dangers of this agent were

recognized, and first found expression in the report of the Royal Medico-Chirurgical Society, in 1864, which formally recommended anæsthetic mixtures. This reaction received a powerful impulse in 1872, from an address before the British Medical Association by Dr. Joy Jeffries, of Boston, and another in 1889 by the address of Dr. Teale before the same body. During the last seven or eight years the *London Lancet* has strongly commented upon the frequently appearing notices of death from chloroform in its pages, and urged avoidance of its dangers. For the latter purpose, it has thrown its powerful influence in favor of the A. C. E. mixture. From that source we learn that this mixture "is now used largely," while it is added that "a fatal accident from its use has rarely been recorded." * In the pronounced diversity of opinion which prevails as to the relative merits and demerits of the two great anæsthetics, and which has existed more or less markedly ever since these agents came into use, I find, therefore, justification for presenting the claims to your confidence of a combination of the two, with the addition of alcohol.

The origin of the A. C. E. mixture has already been stated, or rather its first authoritative presentation. This was by the report of a committee on chloroform, appointed by the Royal Medico-Chirurgical Society of London, in 1864. This committee made a formal and strong recommendation of several anæsthetic mixtures as means of avoiding the dangers of chloroform. One was composed of chloroform, one part, to four of ether; one of chloroform and ether in the proportion of one to two; and another, which is the subject of this paper, which consists of alcohol one part, chloroform, two parts, ether, three parts — the "A. C. E." The proportions are by measure; its constituents should be of the best quality, the alcohol as nearly absolute as possible. This mixture had been recommended and used by Dr. Harley for several years

* March 27, 1886.

previous to 1864, and with him it therefore originated. The committee found in mixtures of the two anæsthetics an agent which produced modified effects of each alone, more efficient than ether, safer than chloroform. Experiment upon animals in their hands showed a far less depressing effect upon the heart than chloroform alone. They especially recommended the combination under consideration, which, with the theoretical considerations in its favor, gave it the lead as compared with the others. Playfair's recommendation of it in labor, when chloroform seems to disagree, has doubtless promoted its use in England, and aided in bringing it into the favor spoken of by the Lancet. Throughout the section of country where I reside it has been used a great deal. Its advantages were presented to the profession in a published lecture delivered by me, upon invitation, before the class of the Medical College of Ohio, in 1875,* and I know many professional friends who use this anæsthetic exclusively. It has been the sole anæsthetic used by the St. Elizabeth's Hospital, of Dayton, from its origin in 1878.† The only other mixture which can rival the A. C. E. in frequency of use is that known as the "Vienna Mixture," which consists of two parts of chloroform to six of ether, which is said to have been administered 8,000 times without a death. Billroth uses exclusively a mixture of chloroform, three parts, ether and absolute alcohol, of each one part, and believes it to be less dangerous than chloroform alone.‡

That union of the effects of various anæsthetics produced by one being administered subsequently to another, as when the patient is brought under the influence of nitrous oxide, and the anæsthesia continued by ether, will not be considered here. It is doubtless the safest method

* The Clinic, Nov. 27.

†NOTE.—The debate upon the paper revealed a far more frequent use of this anæsthetic by the profession of Ohio than the writer was aware of.

‡ Kappeler, Anæsthetica, 1880.

known, but it requires special apparatus and is, therefore, not at all adapted to general practice.

Is the alcohol in the A. C. E. mixture of any value? I have not only heard the question asked, but have known the negative to be maintained by men whose opinion is worthy of consideration. So far as the vapor of alcohol tends to sustain the action of the heart, its presence is certainly no detriment whenever chloroform is inhaled. Yet in view of the presence of ether at the same time, this argument in favor of it need not be pushed. It is upon chemical grounds that the presence of alcohol is justified, the three constituents forming rather a chemical combination than a simple mixture. This fact has an important practical bearing. The objection to the A. C. E. mixture always heard and constantly urged is that the three ingredients evaporate at different rates, and therefore the patient is soon getting the sole effect of one of them, the chloroform, instead of the three combined. The objection is not sound, or is but partially true. There is no doubt that the alcohol exerts a modifying influence upon the rate of vaporization of the other two constituents. This influence was recognized by the committee which originally presented the combination. They especially recommended the A. C. E. as preferable to the other mixtures, "on account of the uniform blending of the ether and chloroform when combined with alcohol, and the equable escape of the constituents in vapor." Ellis, in his work on the "Safe Abolition of Pain" (London, 1866), devotes a chapter to the rates of evaporation of anæsthetic fluids, separate and combined. He states that the rate of vaporization of the united fluids is, in vacuo, equal, and under the circumstances of ordinary administration it is modified. During the past year I called the attention of a correspondent of the *British Medical Journal* who had urged this objection to the A. C. E. mixture, to this view of the influence of the alcohol. He referred the question for decision to the London *Chemist and Druggist* and in

the issue for October 11, 1890, will be found the response of the editor, which is, that "the alcohol would unquestionably influence the evaporization of the more volatile constituents, as the mixture is partly, at least, of the nature of a solution, i. e., ether and chloroform are dissolved by the alcohol." A modifying influence of great practical value may therefore be justly claimed for the alcohol, at least until farther chemical evidence is adduced to the contrary.

I present the special advantages of the A. C. E. mixture, as compared with the two leading anæsthetics. First, in comparison with ether: It is far less unpleasant and very far more prompt in producing anæsthesia. Whatever unpleasantness attaches to the mixture depends upon the ether it contains, and it is less unpleasant than ether, therefore, because less pungent and because the inhalation is of shorter duration before unconsciousness supervenes. The irritating effect of its vapor upon the air-passages, the distressing sense of suffocation it causes, the extreme struggling often occasioned, the long time required to produce anæsthesia, these are the drawbacks to ether as an anæsthetic. I have witnessed some painful scenes of etherization, even in hospitals, under the hands of experienced administrators. I have met with several patients who, for a second inhalation, deliberately chose chloroform, even after fair statement of the relative danger, rather than again undergo the painful sense of suffocation once experienced. In promptness of action the A. C. E. mixture leaves nothing to be desired. In how short a time surgical anæsthesia could be caused by it, I cannot say, for rapidity of the anæsthetic process introduces very certainly a new and unnecessary element of danger. By the mixture patients can be anæsthetized as certainly as by chloroform itself, and as rapidly as it is prudent to do it with that agent. It may be admitted that with the closed-bag inhaler, whereby the patient breathes not only the vapor but his own expired air, anæsthesia can be as

promptly effected by ether as any agent, but in this way the sense of suffocation is increased in intensity, although shortened in duration, and the need of a special apparatus places this mode outside the limits of general practice. Parenthetically, it may be said that experience has not shown the danger to be increased by this mode of administration of ether.

There is but one point upon which to compare chloroform and the mixture. That point is, of course, safety. Were it not for the greater danger attending its use, chloroform would stand without a rival. The proof of that danger, positive and relative, has been so recently ably presented to the profession by Profs. Wood and Hare,* that it is needless to go again over the ground. That, when experimenting upon animals, sudden death takes place far more frequently under chloroform than under ether, does not depend upon the testimony of one or of a few observers, but is the universal testimony of experimental physiologists. That in man the death rate under chloroform is considerably higher than that under ether is the conviction of every one who has studied the subject, although the exact rates can not be given in figures.†

Except in obstetrical practice, this agent has too often shown its fatal power, and no one can be a constant reader of a British medical periodical without being painfully struck with the frequently appearing notices of death under its influence. In the great power of chloroform relatively to ether, greater than is that of brandy to wine, can be readily found an explanation of its lethal effect. It is to lessen and to modify this power that the minor anæsthetic and the alcohol are added. But it is not alone as diluents of chloroform that the other constituents act. They oppose to the constant and never-failing depressing

* Med. News, Feb. 22, 1890. Address at International Congress. Ib., August, 1890.

† See Appendix I.

influence of chloroform upon the heart their own stimulating effect. It is true that under ether, sudden failure of cardiac action sometimes takes place; death has occurred quite as suddenly and as unexpectedly under this agent as under chloroform, but depression of cardiac power is very exceptional under ether; it is a constant feature of the action of chloroform.

We are now prepared to consider what parts or portions of the dangers attending the process of artificial anæsthesia are avoided or obviated by the mixture. In experimenting upon animals it has been constantly observed that sudden deaths occurred in a rapidly increasing ratio as the amount of chloroform vapor carried by the air was increased. In other words, rapidity of introduction, independent of quantity inhaled, has a powerful influence in causing accidents. Clinical experience teaches the same lesson. I think no one can make a careful study of the particulars of death under chloroform without being struck with the frequency with which it is stated that the dangerous symptoms occurred immediately after the addition of more of the anæsthetic to the sponge, napkin or inhaler. Inhibition of the heart from the sudden impression of the vapor upon the terminal nerves of the respiratory tract has been an explanation of this mode of action. But the high authority of Wood has been given against this view.* Still I am not prepared to abandon a theory which has the sanction of Kappeler and of Richardson and of Laudor Brunton. But it is needless to debate the point, because it matters little what the explanation may be, so long as we have the facts. The facts are that when animals are going under chloroform, any fresh administration causes most remarkable variations in the blood pressure and in the heart's action: "There is frequently a sudden dip in the pressure to the extent of forty millimetres out of a total of one

* Med. News, Feb. 22, 1890.

hundred and ten." * Further, that in man, sudden death has frequently occurred upon rapid introduction of vapor into the blood either by deep inspirations or by supercharging the air. Of 133 fatal cases sixteen presented this feature, and in eight of them the fatal effect followed immediately upon the addition of more chloroform to the sponge.† In this mighty influence of the rate of introduction upon the effects of the agent is alone to be found, I believe, a rational explanation of the deaths caused by the inhalation of a very small quantity of chloroform, a quantity sometimes measured by minims, and also an explanation of the occurrence of death in patients who had taken it with safety, not only once but several times before. In the mixture, then, this great and dangerous power of chloroform is held in abeyance. The sudden impression upon the terminal nerves of the air-passages cannot be produced, nor can a large amount of chloroform vapor be rapidly introduced into the blood. Dilution with air it may be said will effect the same results. This is true, but such dilution can never be relied upon, unless the quantity of liquid poured out is measured, and the administration effected by an apparatus which will regulate the supply of air and of vapor. When chloroform is not thus administered, by inattention, by inadvertence, conditions may at any time occur which imperil the life of the patient. The importance of low percentage of vapor of chloroform in the air inspired, of slow and gradual progressive increase in amount, the bearing of the rate of introduction upon the safety of the process have been taught over and over again from the beginning, yet their value has not been duly appreciated; certainly they have not been by those who administer this anæsthetic on a towel or napkin, folded or unfolded, a plan which insures irregularity of amount inspired in a given time. Again, the varying rate of introduction by varying

* Report of Com. of Brit. Med. Association. Brit. Med. Jour., 1879.

† Holmes' Surgery, Am. Ed., III., p. 540.

respiration should be ever kept in mind, and is of the utmost importance when the stronger anæsthetic is given. During ordinary respiration, twenty cubic inches of air are inhaled at each inspiration; under deep inspiratory effort, not only twice or thrice this amount, but as much as one hundred cubic inches may be inspired. The influence of varying temperature upon the amount of chloroform which air will carry is a point in this connection which deserves careful consideration. At 85° F. air will contain and carry twice as much as at 65°.* The rate of inhalation, therefore, may be nearly doubled within a possible change of temperature. Much of the danger, then, which attaches to the administration of chloroform from too rapid introduction of the vapor, and from a violent impression of this powerful agent upon the air-passages, from a possible carelessness of administration, is obviated by its dilution with the other agents, while part of the anæsthetic effect is produced by the ether, and the greater part of the depressing influence of chloroform upon the heart avoided if not antagonized.

The objections which have been advanced to the A. C. E. mixture deserve careful consideration. When first proposed, two were advanced, which, if true, would have prevented this anæsthetic from obtaining any place in practice. They were, that from want of power, as compared with chloroform, the patient would suffer pain; and, the length of time necessary to induce anæsthesia. Neither of these deserve a moment's consideration. Although for all prolonged and severe operations I always give a hypodermatic injection of morphia and atropia, which, of course, deeply modifies the anæsthetic process, still many operations and procedures do not require this, and I have never in a single instance failed to obtain profound and satisfactory anæsthesia. The time required has already been stated to be as short as consistent with safety. Not infrequently I have heard, it is true, of failures to obtain

* Snow. Sansom.

a satisfactory result with the A. C. E., but I am satisfied that when this occurs it is for want of proper use of it. No especial mode of administration is required. The first three golden rules for anæsthetics should never be omitted: First, the apparatus should not only permit a free current of air, but, for all agents except ether, be such that it cannot be cut off. Second, calm and sustain the nervous system of the patient. Third, by small doses at first gradually accustom the air passages to the pungency of the vapor. Then give with a free hand.

There are two pathological conditions in which there is tenable objection to the use of the mixture. These are old subjects with bronchitis, or a tendency thereto, and patients with organic disease of the kidneys. In such cases the A. C. E. is objectionable, as ether is also, and on account of the ether it contains.

The objection most persistently and most frequently urged against this mixture is the different rate of evaporation of its ingredients. This objection has already been considered and shown to be far more theoretical than practical. For many years, however, it influenced my mode of administration. I would accept no other means than a large cup-shaped sponge; with this, a squeeze of the hand upon every fresh addition of liquid, not a shadow of standing-room is left for this objection. A good sponge, of proper shape and size for this purpose, is difficult to find and expensive. Of late years, I have used Allis' ether inhaler and find that, with this, the objection is of no practical force whatever, even in prolonged administrations.

A far more serious charge has been made against this mixture, and one, which, if true, should banish it from practice. A series of experiments upon animals has been published which go to show that this is among the most dangerous of anæsthetics, even far more dangerous than chloroform. Thus the results were, for ether a mortality of 16.6 per cent.; for chloroform, 66.6 per cent.; second

series, 58.5 per cent.; A. C. E., 75 per cent. These are the results obtained by Dr. B. A. Watson.* Looking carefully over the details and results of these experiments, and with a deep sense of the responsibility attaching to a decision upon a most important point of practice, I make the following comments: First, experiments upon animals which corroborate clinical experience are unimpeachable; when the results run counter to it, preference must be given to observation upon the human subject. There has been a large experience with the A. C. E. mixture, an experience which extends over a quarter of a century, and so large that if the mixture were anything near as dangerous as chloroform, to say nothing of being more dangerous, it would, long before this, have been apparent. Second, the conditions of the experiments were such as to destroy the value of the results. To occupy twenty minutes in inducing the anæsthetic condition, to continue the process without break for two hours, to use animals which had been anæsthetized over and over again, is a method so different from that followed at the bed-side, that to compare the results of the one to the other is unjust and illogical. Third, the experiments show too much, if reason is to be permitted at all upon the point at issue. That by adding to chloroform two substances which weaken its power, and both of which counteract its depressing influence upon the heart, it is thereby made more potent for evil, is again illogical, and has not a particle of theoretical or clinical support. Finally, without questioning the ability or honesty of the observer, [than which nothing could be farther from my intention], these results cannot be accepted until they have been obtained by other observers, while they stand in direct opposition to those of the committee of the Royal Medico-Chirurgical Society.

The objection may be advanced against the mixture that it contains chloroform, that this agent has been

* Transactions Am. Surg. Assoc., Vol. II., 1884; An Experimental Study of Anæsthetics.

shown to be more dangerous than ether, and that, therefore, it should be abandoned entirely in favor of the safer anæsthetic. The objection would be unanswerable if the record of ether as to safety was an unbroken one. Unfortunately this is not the case; unfortunately again the ratio of danger cannot be stated in figures; the number of deaths under each anæsthetic has certainly never been given with accuracy; the number of administrations of each it is impossible to determine. Under these circumstances, judgment must be rendered from experiment upon animals, upon theoretical grounds and imperfect clinical results. A careful study of these brings me to the conviction that the mixture is equally as safe as ether, and I act daily upon this conviction in one of the most responsible professional duties of life. But in rendering judgment, the factors of convenience and comfort to both patient and practitioner should not be set aside. In these respects ether has marked disadvantages; it is, as termed by the committee which recommended the A.C.E. mixture, an "inconvenient anæsthetic," and I feel fully justified in considering such points as comfort and convenience until it is positively shown that danger is increased by so doing.

I have made a careful study of accidents under the A. C. E. mixture. I can find but three deaths reported, which are here appended.* That these form a complete mortuary list is not probable, considering the mass of periodical literature to be examined. Cases of dangerous symptoms and narrow escapes under anæsthetics are quite as instructive and as worthy of consideration as cases of actual death. Of these under the A. C. E., there are two to be found quoted in Turnbull's *Manual of Anæsthetics*, third edition, and one occurred to my friend, Dr. Weist, of Richmond, Indiana.† I have found one other reported, and in this chloroform had previously acted very badly, as

* Appendix II.

† Cin. Clinic, Nov. 27, 1875.

in one of my own cases.* My own experience of unfavorable results with the mixture extends to three cases of dangerous symptoms. Two of the patients were young females and one was an adult male. The condition in all was alarming; the two females responded readily to measures of restoration; the male patient seemed for a short period to be lost. These cases are enough to show that the A. C. E. mixture is not absolutely safe. It is not recommended as such. No such anæsthetic exists. The three accidents which have occurred to me have taken place during twenty-six years of active practice. I began using the A. C. E. mixture within a year or two after its first recommendation, and I have used it ever since, except in obstetrical practice and for very young children. I have used it for all sorts of patients; for all kinds of operations, minor and major. I recognize fully how insignificant is any individual experience as to anæsthetics, except under very exceptional circumstances, but personal experience always speaks with a loud voice to the person who makes it, and so I may be pardoned if I have acquired strong convictions of the practical advantages and the safety of this mixture. It is to be regretted of course that to the three deaths published the number of administrations cannot be added, but this is impossible. The mixture is "largely used in England" according to Buxton.† The *Lancet*‡ speaks of "extensive experience" having demonstrated its efficiency and its far greater safety as compared with chloroform. Adding to these statements the fact of a very considerable use of the mixture in this country, and it is certainly safe to conclude that there has been such an experience with it, that a rate of mortality at all approaching to that of chloroform must already have become apparent. I fully believe it to be as safe an anæsthetic as any, and one by which the dangers of chloroform and the inconveniences of ether are alike avoided.

*Med. Record, Nov. 15, 1890.

† Anæsthetics: Philadelphia, 1888.

‡ Editorial, March, 27, 1876.

[illegible]

Statistics [illegible] comparative [illegible] of different anæs-
[illegible] selected during the investigation of the above
[illegible] examination of *London Lancet* from 1881 to 1890
both inclusive, showed —

Deaths from chloroform [illegible]
Deaths from ether [illegible]
Deaths from mixtures [illegible]
Deaths from [illegible]

Among the chloroform deaths [illegible] trachea or pharynx [illegible] shown at autopsy and [illegible] one case of self administration [illegible] suffering great [illegible] given for [illegible] deducted.

The statistics of St. Bartholomew's Hospital for [illegible] years from [illegible] are essential to [illegible] give:

Chloroform [illegible]
Ether [illegible]

Both of the ether fatalities stated to be in very [illegible] condition from prolonged alcoholic [illegible].

Ether produced [illegible] administrations with one death. Thus we have deaths from chloroform, 1 in [illegible] life, from ether 1 in [illegible]. [illegible] 1 in [illegible] 1 in [illegible].

A writer in the *Lancet*, June 21, 1890, gives the results from the Registrar General's office of deaths in England and Wales, from 1878 to 1888, as [illegible] both ether and chloroform deaths are given together, and thus no comparison is established. As showing the varying influence of sex, however, these statistics are important. 150 were males, 74 females. This is about the usual proportion.

Natoe gives number of cases reported in *Lancet* and *British Med. Journal* for three years as follows: Chloro-

form, 1887, 16; 1888, 12; 1889, 13. During the same period, from ether, 3, and from nitrous oxide, 1. [*Lancet*, July 5, 1890.]

The largest statistics I have ever seen have been published quite recently. They are by Dr. Juillard, of Geneva.* He gives 524,507 cases of anæsthesia by chloroform with 161 deaths, or 1 in 3,258; 314,738 cases by ether with 21 deaths, or 1 in 14,987. These statistics also show a fact already well known, the far greater danger of the early stages of chloroform inhalation. Thus, in 243 cases, death occurred in 127, or more than one-half, before anæsthesia was produced. In 20 cases, personally observed, death occurred in the early stages in 15.

APPENDIX II.

Deaths from administration of A. C. E. mixture:

I. Adult male; amputation of the leg for railroad injury under ether, rapid recovery. Later, for removal of necrosed bone, the A. C. E. mixture was administered; the patient died in convulsions; convulsions occurred before anæsthesia was produced.†

II. Adult female, anæsthetized for flexion of anchylosis; knee-joint was flexed and splint applied; shortly after straightening the leg the surgeon's attention was called to the patient; her condition was found dangerous and all measures of relief were put in force in vain. It is distinctly stated that the patient was brought under the influence of the A. C. E. mixture, but in the report the word "chloroform" is constantly used afterwards. As it reads, therefore, the death was from chloroform, but it seems as if the word "chloroform" was used as a synonym for the mixture, and we accept it as such.‡

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*British Med. Jour., April 25, 1891, from Rev. Med. de la Suisse Romande, Feb'y, 1891.

†Amer. Jour. Med. Science, Oct. 1876, p. 415.

‡Brit. Med. Jour., Nov. 3, 1888, p. 1028.

III. Female, age 32, New York Hospital for Women; died before operation began. Autopsy showed both kidneys full of abscesses.§

IV. To these must be added another, reported verbally by Dr. Stamm, of Fremont. Patient, a boy aged about 14, operation for removal of necrosed bone from leg.

DISCUSSION.

Dr. C. A. L. Reed:

I wish to make an inquiry of Dr. Reeve—and I ask him because he is familiar with the literature of the subject: To what extent have statistics been compiled that will show, not the primary deaths—that is, the deaths upon the table—but those deaths which occur from lung and kidney complications through the use of ether? I have long been convinced that many cases of death can be attributed to the use of ether, in which death occurs subsequent to the operation, in periods varying from one to several days. Another class of cases might well be investigated, and that is the case of trouble, for instance, from abdominal operations, in consequence of bronchial troubles, where throat irritation and persistent cough are excited, and they in turn bring about hemorrhage or other deleterious conditions within the abdominal cavity.

Dr. W. J. Scott:

I would like to ask a question as to the kidney business. Ether acts incontrovertably. My experience and observation latterly are, that persons who have kidney complications or kidney disease, if they take ether, afterwards suffer a bad sickness, the kidney trouble being increased; and sometimes the symptoms resulting from kidney disease go on very rapidly after using the ether. And I think the mixture is just about as bad in such cases, and that in them chloroform is better. I would like to know whether the doctor has made any observations on that subject.

Dr. J. McCurdy:

I have tried all mixtures, and I have never had an accident yet, with either chloroform, ether, or any other anæsthetic. I like to use the A. C. E. I don't know that I make it just as Dr. Reeve does. I am disgusted with

§London Lancet, March 1, 1884.

ether and I am afraid of chloroform. I have had more nearly fatal results with ether than with chloroform from the bronchial troubles that were spoken of and the peculiar forms of disturbance, and I have not had them with the A. C. E.; still, I am afraid to use any; but when the time comes, I certainly shall use some such mixture as this paper recommends.

Dr. R. H. Reed:

There are two or three questions that I might bring up, not in the way of objecting to the A. C. E. mixture, because I am not prepared to do that. In speaking of deaths, it occurred to me that in proportion to the amount of A. C. E. mixture that was used, there were perhaps as many deaths occurred, in proportion to the number of operators who have used it, as there have been from ether or chloroform. For instance, as near as I can ascertain, there have been 375 deaths from ether, 52 deaths from chloroform, and 30 deaths from the A. C. E. mixture. We have had a number of accidents in our city. No doubt there have been more or less deaths from that mixture. I am sorry the doctor did not touch on that. I think that is a question entitled to notice, because it is certainly a very dangerous mixture. It has also occurred to me that in the use of the A. C. E. mixture we only lessen the danger—that in proportion as chloroform is used we increase the danger of death; that in proportion as ether is used, we lessen the danger of death. In reference to the time occupied in giving ether, it does not seem to me that twenty minutes is a moderate average. I know with regard to myself that ten minutes is a limit for giving ether, if you give it in a proper manner and use a pure article. As I have seen it given at various hospitals, I can say it is very objectionable. It is given by a great many internes at hospitals by pouring on an immense amount of ether on a cloth, and clapping that down on the face of the patient. Of course it will shock him, and give him a sensation of smothering. If it is given properly, I am satisfied you can take any proper case, give ether, and have the party ready for operation inside of ten minutes. I have done it over and over again.

With reference to chloroform, it is the first anæsthetic I ever used. I was taught to give chloroform as an anæsthetic. But I am afraid of chloroform every time I use

it. In the experiments that I have made on the lower animals, it has seemed to me positive that chloroform increased the danger in the ratio that you used it, and the less you use of it, the less danger you have.

Now with reference to the use of different apparatus for inhaling ether: I have seen a great many different forms of this apparatus. I am convinced that it is not advantageous to use any particular apparatus. For instance, if you use the same apparatus over and over again, the patients are liable to vomit and slobber, which runs over the apparatus; consequently, I think it is objectionable for that reason. The best apparatus that I have ever found is a good clean towel, with a layer of newspaper folded between the layers of the towel, so as to prevent the too rapid evaporation of the ether, and too much inhalation. It is clean every time. It has been the most suitable apparatus that I have ever found.

Dr. H. J. Herrick:

I suppose we might each of us discuss this subject from our own standpoint, and perhaps add something to the merits of the question. I am gratified with the paper of Dr. Reeve. It gives us new light, perhaps, with regard to the subject. I am impressed, though, with this thought: That it is a complex problem, and that a number of factors are involved in it; and it is absolutely impossible for us to arrive at anything very definite in speaking of it from the idea of a mixture by itself. There is the mixture of ether, the method of administering it; there is the patient himself, his condition; we have a variety of forms of the A. C. E. mixture—of chloroform and ether and alcohol—that constitutes one of the problems, complex in its character. Then we have the method of administration—one administers roughly, slovenly, on towels, on sponges without any special appliance; while another administers perhaps through some modern appliance. This complex use of statistics makes them of little value. There is the question of the condition of the patient, the most difficult problem of all. We might go back now to our theories with regard to the use of anæsthetics upon the nerve ganglia, upon the cerebral functions; the question of the action upon the involuntary portions of the body; the condition of the kidneys; the condition of the liver; the condition of the heart; the œdema of the lungs. You

see, the problems that arise in connection with this subject are almost innumerable. I don't say this to attack the paper at all, but simply to call our thoughts to the complex problem—to all of the evils involved.

I will mention a case that came under my observation —a patient that died from inhaling the A. C. E. mixture. On post-mortem there was an enormously enlarged tumor with œdema of the lung. So we find it difficult to arrive at a very definite conclusion in this matter, unless you examine all the factors involved in the problem.

Dr. C. E. Beardsley:

This is one of the subjects that should be thoroughly discussed in this society. It reaches every physician who practices medicine or surgery more particularly than any other question. I would like to hear the opinions of every one here of the action of the A. C. E. mixture, chloroform and ether. Here is a point that I want to bring before the society, and hear their opinions as to their use in traumatic injuries, where you can scarcely feel the pulse of the patient. You use ether, and you find the pulse will rise; whereas, you use chloroform or the A. C E. mixture, according to my experience, you find that it don't rise as it would under the influence of ether alone. Therefore I make it a rule, in administering chloroform, or ether, or the A. C. E. mixture, to administer them according to the condition of my patient. I use ether alone where I haven't a full, bounding pulse. If the patient seems to be plethoric, full of blood, I use chloroform alone. And I always get good results. I have never had any trouble. Where I find the patient is bloodless, anæmic, weak heart, I find ether to operate better than any other anæsthetic. Of course I have found the A. C. E. mixture to operate well in a majority of cases; but I find in those cases where they are bloodless, excited—for instance, in railroad accidents, where the limbs are run over and crushed, I use ether. I had a case of that kind a short time ago where I had to amputate at the hip joint. You couldn't feel the radial pulse. I administered pure ether; the pulse rose right under the influence of it, while the patient was under its influence.

Dr. X. C. Scott:

There is one objection to the A. C. E. mixture: you

have two gases, and there is no certainty in the quantity of either of the gases. I have used all. I have abandoned everything but chloroform. I find that ether or the A. C. E. mixture produces excessive vomiting. I have, for the last eight years, in every case that I have given chloroform, about three-quarters of an hour before I administered it, given a large dose of whisky. Since I have adopted this method I have had no trouble, or scarcely any vomiting, nor do I have any case of heart failure. It is not original with me. The first time I ever saw it administered—that is, whisky—it was done by Dr. ——— of Baltimore. I must say I never knew how to administer it until I gave whisky. Since then I have had no trouble—no heart failure; and it is extremely rare that I have a case of vomiting.

Dr. F. C. Larimore:

I have been an admirer of Dr. Reeve. I have not been impressed with the use of the A. C. E. mixture. I believe in straight ether, straight chloroform, and chloroform for children under ten years of age. I may not gauge the year at ten, eleven or twelve. With children I never have trouble with chloroform. With a patient that I prefer ether, I use the Powers & Weightman ether, and use a towel, as suggested by Dr. Reed, and a piece of paper, and wrap it up in the form of a cone. I leave an opening on for my little finger, and pour the ether on liberally, and put it on the patient's face.

There is one thing that is urged by Dr. Reeve, which I think every surgeon should understand, and every practitioner; and that is, to gain the confidence of your patient. With a little instruction as to inhaling the ether, how it may affect him, that it will produce a smothering sensation, and not bring it down suddenly on his face, you will get along very nicely, and you will have no trouble. I haven't had in five or ten years any trouble except in the Keenan case, and I didn't administer the anæsthetic there. Indeed, I don't use it any more. Prior to that I did. It would require three or four men to restrain the patient while getting under the influence. I used to administer ether for dentists to extract teeth. When I have gained the confidence of the patient, with a few words, I can generally succeed. I don't like the ether. It gets in my clothes, and always gives me the

headache. I very seldom use ether at all, and when I do, I go home with a severe headache all day from it. Still, I look upon it as a safe anæsthetic, and have had no bad effects from it. I shall continue to use it. Of course I tell the patient about eating no breakfast—nothing but a cup of coffee. In that way I avoid the unpleasant effects of vomiting.

Dr. J. N. Stockstill:

I commenced the practice of medicine before we had ether. I gave it some time, and then chloroform came up. I have used chloroform in all my obstetrical cases. That paper states that it is dangerous. I have not found it so. I never have administered it, without it was administered on a full stomach, with but very little bad effects from it. The late Dr. Keith came to my office in the evening, and wanted a tooth pulled. Says I, "Doctor, I can't pull that tooth without chloroform." Says he, "I have got it with me." He was a druggist, and brought it along. I administered it, and I felt a little alarmed at his condition; but I got hold of the tooth and pulled it out. Come to find out the doctor had loaded his stomach with a pretty heavy supper before he came up. This gentleman on my left speaks of stimulants. I have never used them, but will use them now.

Dr. D. H. Brinkerhoff:

Like my friend who preceded me, I have been practicing forty years, and I have been in the habit of using chloroform from its first introduction. So far as my own experience is concerned, I find chloroform very much more efficient than ether. In many instances where I have tried to use ether, before I could get the desired effect, I have had to resort to chloroform. I have had no difficulty in combining the two at the same time with the same patient. I have not yet, so far as my experience goes, had any ill result from the use of chloroform, nor have I had any ill results from combining the two. I sometimes combine them, and sometimes use one, and sometimes use the other.

Dr. N. Gay:

I don't rise to discuss this paper, but simply to report.

a case of death, some ten hours after using ether, evidently the result of ether, when the man had completely recovered from the ether. He died from congestion of the lungs about ten hours after he had taken the ether. That is the only case I have ever lost from the effects of an anæsthetic. This man was a German, very much subject to drinking beer. He had a rupture about two days before I gave him the ether, and operated. I returned the bowel to its place. He had a stool within about an hour; subsequently another stool, and then he went to sleep, and woke up about four or five hours afterwards with a most intense difficulty of breathing. I was called, and I immediately did everything in the world to relieve him, and bring him to, but the congestion filled the lungs, and they became perfectly solid with blood, and he died. That is the only case I have ever seen reported—the only one that I know of—of death after an operation, and after he was apparently well—from congestion of the lungs following the use of ether. I never would operate on another case of hernia while a man was vomiting, and use ether. I operate with chloroform in that class of cases; I believe it is safer and better. The doctor speaks of giving whisky. That was the old treatment used throughout the army. There were very few losses. They were given whisky about thirty minutes before the operation, and then given the chloroform. I never saw a bottle of ether while I was in the service.

Dr. J. F. Baldwin:

Individual experience with any anæsthetic, unless under extraordinary circumstances, is almost entirely valueless. I know of a practitioner who enjoyed a very large general practice, but who never had a breech presentation. I had an experience a few months ago, which I hope no one of this society has had; I had a death under chloroform. The patient was a woman about seventy years old, who as her physician informed me by letter, had had cystitis for 30 years, and who could not be examined at all without an anæsthetic. Her physician was sick, and unable to be present to give the anæsthetic. I had a young man with me, of two or three years' experience, who had given chloroform for me before. I saw some of the woman's urine in the chamber. It was alkaline, evidently, and contained blood, and with a history of some thirty years of cystitis, in accordance with all modern doctrines, I did

not give ether, but gave chloroform. I had both with me, but regarded the chloroform as the safer. She took the anæsthetic well. I examined the parts, and found a large urethral caruncle, which I removed. I completed the operation, and my assistant stopped the chloroform while I washed out the bladder with warm water. It was, so he said, at least four minutes after he ceased giving chloroform when he noticed that respiration had ceased and that the patient was getting black in the face. He called my attention to it. I turned her head downwards, and resorted to artificial respiration. In a very few minutes she commenced breathing. She took four, or five, or six good healthy inspirations, and then stopped, and she did not breath any more. I kept up artificial respiration, but the cyanonis deepened; the entire surface became livid. As it has been claimed that, if in these cases, we prick the heart, the heart's action will be excited, I snatched up a delicate exploring needle, and thrust it into the heart, and found it still acting. I still kept up artificial respiration, and still kept the patient suspended. The heart's action continued thus for fifteen minutes, and then it stopped. No further attempt at breathing had been made. The patient was dead. I do not wish my worst enemy any worse punishment than to meet with a similar accident.

Some considerable time had elapsed after the use of the chloroform before the unfavorable symptoms arose. The patient did not die according to the Hyderabad Commission, because she continued to breath; she did not die according to H. C. Wood, because the heart's action continued, and it continued for at least fifteen minutes. We had no post-mortem. We might have discovered an apoplectic effusion, or something of that sort.

I at once wrote an account of the case—as a hypothetical case—to Dr. Reeve, and asked him what anæsthetic should be used. He replied at once chloroform, owing to the undoubted damaged condition of the kidneys. I then wrote him that the case was an actual one, that chloroform had been used, but that the patient had died. He replied that, nevertheless, chloroform was the proper anæsthetic to use in such a case. I was very glad indeed to have the sanction of a man so eminent in the matter of anæsthetics.

I never have had any other serious effects from the use of

chloroform, though I have used it almost to the exclusion of ether. I have had this death; I may have another; but I have still used chloroform, but not with the degree of confidence that I did before. I think I shall hereafter use the A. C. E. mixture, except in obstetrical practice, or in cases where it is contraindicated. Years ago I stopped using it for the reason that Dr. Scott assigned that the fluids would not evaporate with the same degree of rapidity; but it has been shown that they do evaporate alike, and I think that I shall turn over a new leaf and take up the use of the A. C. E. mixture.

Dr. Larimore speaks of giving chloroform to children with impunity. That used to be taught, but a few years ago a fatal case was published in the Cincinnati *Lancet*, and statistics then given of the deaths that had occurred from the use of it in children. I question very much if children have any immunity in this respect.

I am glad that Dr. Reeve has said what he has as to its use in obstetrical practice, because there ether cannot be used.

Dr. S. S. Thorn:

I have used ether, chloroform and the A. C. E. mixture, with periods when I have used one to the exclusion of the other. I use now in my own practice, except where some other man is responsible for the well-being of the patient, the A. C. E. mixture, and nothing else, or practically nothing else. I presume I may safely say that I have used the A. C. E. mixture 5,000 times. I think that I am within the limits. I have used it for twenty years, I am sure. I want before I go any further to say that, if I had a patient who had a face that was congested, as I understood Dr. Baldwin to say, I would not stand that patient on the head. While that is the general rule for the care of patients who are suffering from anæsthetic poisoning, I would make that exception. It may not have any force or effect, but that is the way it strikes me.

As to the use and choice between ether and chloroform, the demonstration of Schiff, of Geneva, was enough for me. From ten thousand or more of elaborate experiments in the investigation of this question, he decided that one losing a patient from the use of ether should be held responsible. If he is held responsible for the death of a patient from the use of ether, how much more must the responsibility be for a death from the use of chloroform? I don't want a better demonstration than the re-

port of those experiments. I am satisfied in my own mind as to the value of the two agents. With me and in my house, I shall continue to use the A. C. E. mixture. Somebody asks, what is the A. C. E. mixture? I don't know whether Dr. Reeve gave it. A. C. E. are the factors in the case. They not only include the factors, but they give the proportions of the agents—A. being alcohol, C. chloroform, and E. ether, in the proportion of one, two, three. I believe it is an important matter to have this proportion maintained. In a large number of experiments, aside from my regular practice in the use of the A. C. E. mixture, upon animals, I satisfied myself of this. If you ever had a dog on the table, you know how you began to pull out the tongue, and how you would work with that dog. I never have found it necessary to pull a dog's tongue out under the A. C. E. mixture. In my little sphere that was demonstration as to the value of this agent. Alcohol is an important factor, because I think that it equalizes the two gases through the juncture, as my friend Scott says. They do not evaporate separately by reason of the presence of alcohol. Therefore when we give the A. C. E. mixture, we have the combination in the exact proportions, and hence it is valuable. It is held so by the alcohol. If this separation does obtain, then we have the chemical effect of the alcohol and the conjoined effect of the A. C. E. mixture. Somebody objects to the use of ether and chloroform, because the ether goes away and leaves the chloroform: you will have chloroform, and that in an uncertain quantity. There is no cast-iron rule for the administration of an anæsthetic, which has been demonstrated here to-day. One man gives it, and another would not give it in a case of œdema of the lungs. You have got to have some science even in the administration of an anæsthetic. There is no cast-iron rule that will do that. I could not construct a set of rules which might obtain, or that would be a guide in all cases. A man sits down and says, "Dr. Thorn writes so and so, then you must move thus." With all the rules that are written, you must have your head. So it is with the A. C. E. mixture. The A. C. E. mixture can be used in smaller relative quantities than any other agent we have. We use much chloroform, much ether, and much A. C. E. mixture, dependent on the method of administration. The A. C. E.

mixture may be given to children just as well as chloroform, without any single exception; it may be given to people in young life; it may be given in middle and active life; it may be given in advanced life. I have at times given it to an infant when born, and a few hours old, and I have given it to an infant of 90 years and four months; and do you believe it? the old man would hardly lie in bed long enough to wipe our instruments. I would not hesitate to give it to a patient upon whom I was to perform an operation. That is the way I give an anæsthetic. If you have made up your mind to make an operation, then give the A. C. E.

A word about the use of chloroform. Schiff demonstrated that it had an influence on the heart; that it was a sedative, and a positive sedative. My friend says, ether will lift up the pulse. Schiff demonstrated that to the satisfaction of anybody who can read.

I know that there are some advantages following the judicious use of the A. C. E. mixture. It is without danger; or as nearly so as any anæsthetic that has ever been used. I said to the gentlemen of our society a number of years ago, "Gentlemen, I am not going to use chloroform any more." Some of them laughed at me. Some of them said "I guess he don't know how to use it." Others said, "Oh, yes, he does; he has used it as much as any other one of us." Two of the men who laughed at me in about two months had occasion to sign a little bit of a certificate about the size of my hand, which we are obliged to sign after each death. I have never had any unpleasant results from the use of an anæsthetic since I began the practice of medicine.

Dr. T. A. Reamy:

I have been unfortunate. I was informed that you had adjourned until 10 o'clock. I waited, and so I did not hear this paper read. I said in a meeting in Hamilton not long since when this subject was under discussion, that I had read everything that had been written by Dr. Reeve on this subject; and I was hoping that I would be here, not only to hear him read this classical paper, but to hear him defend it, with an ability which is not surpassed by that of any man in the defense of his paper. I have been deprived of that privilege. Then I said further, I wanted to know what excuse a man of his great ability

and his thorough knowledge of what had been written upon the subject, as well as a large clinical experience, had for advocating such a remedy as an anæsthetic.

My friend, who is a high convert to the A. C. E. mixture, cites the experiments of Schiff, which experiments, as he says, lead to the conclusion that chloroform destroys life by the paralysis of the cardiac action, and that ether destroys life by the paralysis of the respiratory tracks. These experiments have been modified. I was surprised that my friend was convinced by such physiological experiments, and then he adopted the remedy which must embrace the dangers that must exist in either one; unless it is upon the ground which I understand Dr. Reeve takes, that a new agent is produced, that a new chemical combination is produced. If he takes that ground, he must wait for the results of physiological experiments, and so must the author of this paper. He must wait until the experiments have been made that sustain that proposition; and these experiments have never been made. I have read the experiments that have been made upon this subject, and they are not convincing, they are not conclusive. Their position is not sustained by the physiological experiments. Therefore what proof have we that this agent is better than ether alone, or chloroform? The personal experience of the gentlemen who have used it, as a gentleman over there said, what is that worth? Is it worth anything—the personal experience of any man on this floor? My own is not worth anything, and I suppose I have administered and seen ether administered as often as any man on the floor. I have averaged it ten times a week, for many, many years—sometimes five or six times together. I gave chloroform years ago; I had no death. That doesn't prove that there is no danger in chloroform. I gave it to children, to adults, and I gave it to persons to enable a dentist to extract teeth. I had no death. I have had no death from ether. I never saw a death from chloroform; I never saw a death from the A. C. E. mixture. Therefore my experience don't prove anything. You must rest it on the physiological experiments of the action of the remedy. And to rest it on experiments will require years. What does it prove that this patient died, or that old woman died? Why would she not die? He stabbed her in the heart; perhaps that made her die. No proof of chloroform. She certainly didn't die from

an anæsthetic. There isn't any evidence that my friend when he thrust that needle into the heart didn't inflict a mortal wound. I don't say that he did, but there isn't any proof to the contrary.

Why do you give the A. C. E. mixture? I want to hear some argument. I am anxious to hear the closing remarks of my friend, because I have no doubt he will say something that he has not done in his paper. Why would you give it instead of ether to an old woman with a bronchial irritation? Or why would you give it in a case where you have degeneration of the kidney? Isn't any anæsthetic dangerous in such a case. Take a simple obstetrical case, why would you give the A. C. E. mixture more than ether? I will take ten men in this house and put them under the influence of ether so they don't know anything—surgical anæsthesia; and I will give a banquet for the crowd before we leave the town, and pay the bill—put them under ether in four minutes. I will average four minutes. You can make your own selection of the ten men.

Dr. Reeve:

I will take you up, and I will be one of the ten.

Dr. Reamy:

Is it because you can't bring the patient under quickly enough? No man who hasn't had a prejudice created against it by some new-fangled chemical combination—a kind of a shot-gun; no man who does not recognize that the more care that is taken the safer it is, and the sooner they will get on, will succeed with it. How, or what kind of an apparatus, don't make any difference. Let them have a few whiffs—two or three inspirations—put them under as quickly as possible—the more quickly the safer! It don't make any difference whether it is ether or chloroform. Any man who anæsthetizes a patient by that rule will kill twenty-five per cent. of his cases. I say distinctly, in so far as chloroform enters into this combination, just relatively so far as you give chloroform rapidly you create danger; if you administer ether rapidly, you remove danger. What difference is there? They have a similar power. Yes, that is true; but that has nothing to do with it. Why, you cannot advocate chloroform, because it is not as safe as ether, as physiologi-

cal experiments have demonstrated. For surgical work it is not so safe, and that is the objection. That is the reason I would not give it. That is all there is of it. Why wouldn't you put your patients under alcohol? It produces the same effect. But these experiments say that the ultimate effects on the cardiac and respiratory organs are identical with chloroform—ether identical with chloroform. I want to say that I believe most of the men here can give chloroform, or the A. C. E., or ether with safety, in almost all cases; that the safety will consist in selecting the kind of a patient. My objection to the A. C. E. mixture is that you can't discriminate. Take the chloroform in the case like the doctor's, or ether in another case. I say, select the cases. My friend saying that chloroform is the thing for obstetrical practice is a concession of the whole field. I never was an advocate of the theory that there was some mysterious nervous force in the parturient patient that protects her from danger from an anæsthetic. I would to God that that were so. But, as I say, that is a concession of the whole question.

Let us go on and give ether. That is safer; that will produce as much and as profound an anæsthesia; and you will know what you are getting. When you get a case of suspension of the heart's action, you know what you are doing. Dr. Wood say digitalis is a stimulant of the heart's action. It would have been better that my friend had given digitalis in that case than to have thrust his needle into the heart, in my opinion—or to have done both.

Dr. Reevé:

I regret that my friend, Dr. Hyatt, as no doubt every member of the society does, could not have followed with his paper, then we would have had the whole subject before us, with perhaps more facts on the subject then we have in this desultory shape. I must say that only two gentlemen have confined themselves to the subject of which my paper treated, viz., the A. C. E. mixture. However, I will reply to some of the questions that have been made.

First, in regard to the conditions under which ether is not the best anæsthetic to be chosen. Two pathological conditions have been established, I believe, clearly, from reason and experience, and have been ac-

cepted, I think, during the last ten years as being the conditions under which ether was dangerous to the patient, and not the best anæsthetic to use. Those two conditions are, chronic diseases of the kidney, which was first pointed out by Dr. Emmett, of New York; the other, chronic bronchitis. In either of those cases I should prefer chloroform as an anæsthetic. And for that reason, I advised, in the case related by Dr. Baldwin, the use of chloroform. The same objection obtains against the A. C. E. mixture as against ether under these conditions.

One or two other points I will just touch upon, not to occupy your attention any longer than necessary. The mixture used by the dentists under the name of vitalized air has been brought before you. I think that ought to be mentioned. You ought to be warned against it. Vitalized air is chloroform, and nothing but chloroform—chloroform diluted with other agents, and flavored with some sort of perfume—the oil of cloves, or something else. It was brought to our town, and I inhaled it, and I am satisfied it is chloroform, and only chloroform; and you will have all the accidents from it that you have from chloroform; the only element of safety being its dilution in the form in which it is administered. I speak of it, because we are often consulted in regard to taking an anæsthetic for pulling teeth. You may just as well know, when you are asked about taking vitalized air, that you are asked about taking chloroform.

I am asked if I still employ hypodermic injection in giving A. C. E. I have written a paper upon that subject for another society, which will be read during the present season. I believe it to be one of the greatest improvements, and one of the most important modifications of the artificial anæsthetic processes.

An objection has been made in regard to vomiting. We have laid it down very positively that following a certain line of conduct in giving an anæsthetic prevents vomiting; but it is like a great many other things, it depends largely upon individual idiosyncracy. One rule: Your patient will vomit in proportion to the amount of anæsthetic inhaled. If you give a large quantity of the anæsthetic, you are pretty certain to get vomiting afterwards. To give alcohol previously, to prevent vomiting, is a theory absurd and practically useless. Alcohol and an anæsthetic mixed are too nearly alike in their

chemical combination and physiological effects. You add another dose of hydrocarbon to be gotten rid of. I would say to this gentleman who stated so positively that if you give a dose of whisky you will be certain to have a safe anæsthesia, that I will promise to produce to you from clinical records at least a dozen, if not twenty, cases of death reported from chloroform preceded by alcoholic stimulants. It is a delusion. I base my opinion, not upon personal experience, but upon a very careful study of every case that I could get or read of. I will cite over and over again instances in which death has occurred where alcoholic stimulants have been given.

So I believe that the opinion that children are free and absolutely safe under the administration of chloroform is also a delusion and a snare. The gentleman last on the floor gave me, in manuscript, the case of a child two years old, one of the nearest possible escapes from death from chloroform, in an operation on the appendix. I have given statistics for eleven years of every case of death that has been reported in the *London Lancet*, by different anæsthetics. Of sixty-seven cases of death by chloroform in eleven years, more than twenty per cent. were in children under fifteen years of age—yes, and a good many younger—three, four or five years old. There are elements of safety for chloroform that do not exist in adults. Just so there are elements of safety in the administration of the A. C. E. mixture that do not attach to chloroform alone.

Now this matter of death from chloroform, as has been said before, and I repeat it again, is a very complex problem. It has been very much hampered in its solution by two errors; first, that there is death under an anæsthetic only in one way. That is a mistake. There have been deaths in different ways, both under ether and under chloroform. Death has happened by heart failure; death has happened by trouble with the respiration; and death has happened by sudden and simultaneous paralysis of both. To hold that death is occasioned only in one way is to put back the hand of progress in science; it is to put an obstacle in the way of the solution of this question. And the other error that is constantly pushed forward, is individual experience, which has been very well stated here to be as nothing in the question of anæsthetics. The field is too wide for any man to get up

and say, "I do this, and I do thus and so, and I have not had an accident, therefore it is safe." That is all nonsense.

The gentleman who has made any argument positively against the A. C. E. mixture was last on the floor. I appreciate his eloquence, as you all do. You know he can always make a critical, eloquent, and telling speech; but to reply to him I would have to read the paper over again.

Dr. Reamy:

I didn't hear the paper.

Dr. Reeve:

And I know that the society would not submit to a second reading of the paper. There are just two points open, in regard to which I recommend the young members of this society here present to-day—brought up by my eloquent friend from Cincinnati—to investigate. They need further investigation. The first belongs to the chemist. The chemist should certainly show experimentally, by laboratory experiment, in regard to the rate of evaporation of these three ingredients together. Do they evaporate differently, or do they evaporate as a combination? I have told you in the paper that they evaporate equally, or nearly so. But I say we are open to further investigation. In the next place, I want further physiological experiments upon animals. The conclusions which were arrived at by Dr. Watson, as read to you from the transactions of the American Association, are not sufficient. I told you why they were not, and why it is not safe to follow them. The gentleman last upon the floor made a remark about the theory of the Chirurgical Society, which I presented to you. They speak strongly in favor of the A. C. E. mixture. When two sets of experiments by two different sets of men don't agree, let us have more. That is another point that I present to the younger members of the society; let us have some more physiological experiments, and we will have something to decide from. These clinical experiments—we are asked by the gentlemen from Cincinnati, if these have anything to do with it—physiology alone is to decide.

I am surprised to learn how wide and great has been the experience with the A. C. E. mixture. I am gratified, because it is in accord with my experience and sustains

the conclusions to which I have arrived. They may not be of much value, but, as I said, they are experiences of a lifetime—that is, of more than twenty-five years. What that experience is, I present to you. But do not let any man come to you and say, however eloquently and however pleasantly he can put it, "this is all nothing, because there has been no experience." There has been a great deal in this country and in this state and in England, and that experience has been very largely favorable to the use of the A. C. E. mixture.

Dr. Reamy spoke of digitalis. I have no faith in digitalis, because I think it is a too slow acting remedy. It is all right theoretically, but you can't get its effect in time.

STRANGULATED HERNIA.

BY DUDLEY P. ALLEN, M. D., CLEVELAND.
Visiting Surgeon to Lakeside Hospital and Charity Hospital.

A malady so commonly met as that of strangulated hernia would seem to deserve little attention at the hands of this society. It is, however, for the purpose of causing, if possible, a discussion that I have selected this subject.

In a considerable experience in caring for cases of strangulated hernia, it has been my fortune to succeed in reducing very few without operation. This is because I have seen but a single case in which taxis had not already been tried. In a few other cases, taxis had been attempted without an anæsthetic, but in most of them with an anæsthetic. Most of the cases which I have seen have been strangulated at least twenty-four to thirty-six hours; many of them longer and some up to five days. It is for this reason that taxis has been unsuccessful in my hands, and for this reason, also, that in many, taxis has been omitted as dangerous and the operation of herniotomy undertaken at once.

I have had the misfortune to lose by death five cases. One of these cases was almost moribund when placed upon the table. A second was of an old and feeble man, in whom strangulation had existed for a considerable time, and he died apparently from exhaustion a few days after the operation. Three cases died in from three to four days after operation with signs of perforation. These three were operated at night, under unfavorable circumstances. In one of the three, post-mortem examination showed death to have been from perforation. Although

in the other two cases no post-mortem was held, the symptoms were so characteristic that I think there can be no doubt that perforation took place.

Since all the cases I have seen have been those in which I have been called in consultation, it has interested me to obtain the views of the various physicians whom I have met as to the treatment of strangulated hernia. A common expression which I have encountered has been that the case in question was the first in which the physician had ever failed to secure a reduction of the hernia, so that I am convinced that the commonly existing opinion among the profession is that strangulated hernia is usually, if not universally, subject to reduction. As a result of this commonly existing belief, I have been at times surprised to find the long time allowed to elapse before definite and positive measures for the reduction of the hernia have been undertaken.

A noted French surgeon has said that in hernia there are two indications for treatment. The first is to reduce at once the hernia, and, failing in this, to perform herniotomy. The practical question with us to decide is what effort shall be made to reduce hernia before an operation is resorted to, and how long these efforts shall be continued.

What efforts, then, shall be made to reduce hernia before resorting to operation? I find that the almost universal opinion is that hypodermic injections of morphine are of the greatest service. Some men believe in injecting a fourth grain of morphine close to the seat of strangulation. Others inject the same amount without any reference to location. Some, in an adult man, would inject at once an entire grain of morphine, and if this were not sufficient to allay entirely all spasmodic pain, the dose would be repeated in the course of one hour. As applications to the hernia, it is the practice of some to use warmth; others place upon the hernia an ice bag. So high an authority as Sir James Paget, of London, strongly

recommends that in cases of strangulation, before an attempt at reduction is made by taxis, the patient should be placed in a warm bath for a sufficient time to relieve all spasm. All believe in placing the patient in bed, allowing him to rest after these methods have been employed, until pain shall have ceased and the muscles become relaxed, then taxis is applied, by some in the dorsal position, with the pelvis raised, and the knees flexed; by others, with the patient in the knee-chest position. Doubtless these methods all have their value in certain cases, but there are cases to which they are not applicable, or rather, in which it would be improper to lose time for them to be tried. Cases of strangulation which have already existed for a considerable time, and in which the evidences of serious strangulation are beyond question, should not be subject to the danger of delay involved in the employment of the measures already mentioned, but should be operated at once, for the reason that by the delay, a gut already damaged may, by the loss of time, receive irreparable injury. In cases, however, where the surgeon is called early and where the seriousness of the symptoms of severe strangulation are not excessive, it is doubtless proper to employ the methods mentioned.

How long, then, shall these methods be tried? To say in a given case how long it is possible that the strangulation may exist without serious damage to the gut is impossible. There are, however, certain rules which it seems to me may be followed with a considerable degree of safety.

In the length of time which can be taken for a paper of this sort, it is evidently impossible to discuss in detail all the differences which may occur in various cases, but we may in a word state what would generally be safe. In cases which have existed but a few hours, unless the violence of pain, vomiting and depression are great, it is improbable that there would be serious damage to a gut inside of from six to twelve hours; and six to twelve hours

are certainly sufficient, and more than sufficient in most cases, for an attempt at the reduction of a hernia. At the end of this time if the hernia has not gone back, as I am told it does in many cases under morphine and rest, then taxis should be attempted. This should not be too violent. If taxis does not succeed without an anæsthetic, it is my belief that an anæsthetic should be given and taxis attempted again. If then reduction does not result, herniotomy should be performed at once.

Though this is a safe line of treatment, it is not the line which personally I should prefer, or consider wisest. It seems to me that the sooner a strangulated hernia is reduced, the better the chances of the patient. My own preference would be, immediately, upon being called to a hernia, carefully to attempt reduction by taxis without an anæsthetic; failing at this, I should prepare at once for herniotomy, anæsthetize the patient, and attempt reduction. If reduction should fail, without further delay (having secured the consent of the patient to operation before administering the anæsthetic and attempting taxis) I should proceed at once to operation.

It seems to me that the position which abdominal surgery occupies today, while it may not warrant one inexperienced in this department of surgery to proceed in so radical a method as one accustomed to the operation of laparotomy, it is my belief that one experienced in this department will do far more for patients by radical methods than by any other. The dangers of herniotomy are extremely small—far less, I believe, in experienced hands than the dangers of delay.

The old method of operating upon hernia, and reducing the gut without opening the sac of the hernia, is no longer necessary, and we may now cut down, open a sac, examine the intestine and return it without fear of sepsis. By so doing we avoid in a great degree the dangers of serious injury to the walls of the intestine produced by long delay, and I am fully convinced that the dangers

from perforation caused by delay are vastly greater than the dangers of herniotomy. Almost the only cases which have given me difficulty have been those in which the gut has been strangulated for a long time, and in some of these it has been extremely difficult to decide whether to return the gut to the abdomen or to form an artificial anus. In those cases in which my operations have been followed by perforation, the surface of the gut has appeared to be as good or better than in some of the cases which have recovered, and it is for this reason that it has been returned to the abdomen. In the case which was examined after death, it was found that the seat of perforation was where the gut had been pressed upon by the ring. I think beyond question this is a point which, as a rule, is most subject to damage, and it is a point which should be examined in all operations. The cases of which I speak, in which perforation took place, were all operated at night with poor light and under circumstances where it was almost impossible to examine the gut carefully, and where had it been found to be impaired, a resection and suture of the gut would have been impossible. The only alternative remaining in these cases was to return the gut, or to form an artificial anus. The circumstances of the patients were such that death would have been almost preferable to the latter alternative, and, since the surface of the gut looked sufficiently healthy to permit of reduction, the gut was not pulled down to examine the portion constricted by the ring, since it seemed, all things considered, preferable to take the risks of the reduction of the gut.

If, however, as I have suggested, physicians universally were prompter to act in cases of strangulated hernia, and would not delay radical treatment for so long a time, I believe the cases which would not recover are extremely rare.

There is another reason for a radical treatment in a case of strangulated hernia. When the hernia is re-

duced by taxis, the condition remains unaltered; at any time the intestine may descend and strangulation be renewed. If an operation is performed, the radical cure of hernia can be added to herniotomy with large chances of permanent relief of the patient. It has been my practice, in every herniotomy which I have performed, to add to it the radical cure, and the success which I have met has been most encouraging.

At the present time I am unable to give an exact statement of the condition of the patients on whom the radical operations for hernia have been performed, but I have followed enough of my cases—and the worst cases—to satisfy me that the radical cure for hernia, if well done, is a successful operation in a large majority of cases. Perhaps some members of this body may remember a case of radical cure of hernia which I presented to this society three years ago at Columbus,—a man whose inguinal ring was so large as to permit the invagination of the scrotum with insertion of the wide open hand into the inguinal ring as high as the umbilicus. This man, ever since the operation, which took place seven years ago, has been a puddler in an iron foundry, and I can imagine no severer strain to which the operation for hernia can ever be placed than in this case. The hernia was of a size rarely exceeded; the ring was the largest I have ever seen. The labor which the patient has performed since the operation is of the severest sort. He has never worn a truss a day, and the operated side has remained absolutely intact.

I know there is a vast difference of opinion as to the result of the radical operation of hernia, but I strongly suspect that the results depend largely upon the methods employed in operation. That method which has served any man well is one in which he is liable to believe, and for that reason I have come to employ and rely upon a method which closes the abdominal ring by granulation. But quite as important as the method of closure, I find

to be the fact that when the open method is used the patient is or should be kept in bed for a considerable time, until the cicatrix has become thoroughly solid, and as a rule, I have kept my patients in bed from five to six weeks after operation.

In the radical cure, both in cases which have been strangulated and those which have not, I have employed the various methods which have attracted the special attention of surgeons, but it has seemed to me that the essentials of the operation are, that the sac should be drawn well down before it is divided from the peritoneum, in order, as much as possible, to obliterate the depression above the hernia, that the peritoneum should be thoroughly loosened around the sac so that when the walls of the aperture are drawn together they may have a broad surface for adhesion. The method which I prefer for closing the rings is to pass through the rings interrupted sutures of silver wire. Over the ends of these sutures are slipped spirals of silver wire, and shot are pushed down above the spiral, and the pillars of the hernia are thus drawn into apposition. The length of the spiral is from one to one and one-half inches, according to the depth of the incision and the amount of adipose tissue of the patient, the object being that when the sutures are to be removed, it can easily be accomplished by simply cutting off the shot, removing the spiral, seizing the extremity of the wire suture, which by this method stands up plainly in sight, and removing it. Formerly, for this purpose, I used the interrupted silk suture, but the difficulties of its removal are much greater than with the silver, so that the wound is sometimes kept open longer than is necessary, whereas, with the silver the suture can be removed at the discretion of the operator. My plan is to withdraw these sutures in about three weeks after the operation, and, during the whole history of the wound, simply to hold it open by packing it with iodoform gauze. I do not know that this method is better—perhaps even so good, as that of

McBurney, of New York, but it is essentially the method which I have employed for seven years, and has served me in such good stead that I am loath to abandon it.

To summarize, then, what seems a wise treatment of hernia, I would say that in case of hernia, it is safe to attempt reduction by the use of opium and other aids for strangulation which has existed during six and perhaps twelve hours. That to delay beyond this time subjects the patient to risks greater than those of herniotomy. For those thoroughly conversant with abdominal surgery it seems to me that even this delay is unwise and my preference would be, after having failed at reduction by moderate taxis without an anæsthetic, to administer at once an anæsthetic and make a gentle attempt at reduction; failing at this, to proceed at once with herniotomy, and to couple with the reduction of the intestine the operation for radical cure.

INFLUENZA.

BY D. N. KINSMAN, M. D., COLUMBUS.

Two hundred and forty years ago Salius wrote: "This disease spread over Europe and to the nations beyond; called by various names by the vulgar, it everywhere had the same nature.

"It began with a fever, severe or mild, heaviness in the head and a severe catarrh which descended to the chest. There arose troublesome coughing with thin or thick expectoration, frequently scanty or absent. There was loss of appetite, with depraved or abolished taste. This continued many days. There was marked languor and great bodily weakness, with slow recovery. The fever usually subsided the fourth day or before, but the cough lasted much longer. All recovered except the feeble, the sick or old, those with lung diseases, catarrhs, or those who were poorly fed, or enfeebled by irrational modes of life." This is as true a description of influenza to-day as then. Its lines may be made stronger but their direction needs no change.

Influenza, "the influence," grippe, mode fieber, or "fashion fever," "lightning catarrh," and many other terms have been used to designate this often-appearing, widely-prevailing malady.

It spreads from west to east, and from east to west again. It is confined at times in narrow limits, at others it belts the earth in its march and becomes pandemic.

Neither climate nor latitude, cold nor heat, seems to influence its course, for like death it has all seasons for its own.

Of unknown etiology, frost, rain and drouth, miasms, corrupted air, deficient ozone have all been considered causal factors, as well as the asthmos ciliaris, clouds of insects and the ever convenient microbe.* Telluric and atmospheric disturbances have been incriminated. Deemed contagious, contact does not produce nor does isolation prevent it. Declared by all observers not to be deadly, it doubles mortality lists while it prevails.

If we may trust history, the course of influenza has always been paradoxical. At Rome, all died who were bled. In London, all died who were not bled.

Sometimes it hesitates in its course, at others it overwhelms a continent at a blow. At times it prevails as an epidemic, at others it becomes epizootic as well. Known from the ninth century, it was first clearly described in 1580. It has since become so common that during the nineteenth century epidemics have occurred every decade. It spares no age and respects neither sex nor condition.

In 1732, the disease appeared in New England, spreading thence to the West Indies and Mexico. Fifty years later, as the morbus Russicus, it spread over all Europe and America, attacking ships in mid-ocean in its course.

In 1832, influenza preceded the invasion of cholera in America, and ten years later was known in the United States by the facetious Whigs as the "Tyler grip." This was the most serious attack, until the present one, from which this country has suffered during the present century.

*A member of the English Parliament tells what he knows about the grippe. You are at once seized with the symptoms of catarrh, pernicious and hay fever combined. You feel a sheet of white hot iron enveloping your sides; it seems as if some one was boring into your back between the shoulder blades with a gimlet. You imagine you are being reduced to a soft jelly, if your mouth does not claim your attention by having each individual tooth in a rage. But the "demnition total" of physical suffering is as nothing in comparison with the moral state. You are conscious of having been unjustly condemned, without a hearing, for all the crimes in all the penal codes which have ever been known of.

Epidemics have spent their force in a few weeks; on the other hand, they have, as in our present epidemic, lasted through an entire season and prevailed the following year.

There are no prodomes.* It has seemed to me that a simple coryza was the precursor sometimes, but even then it is not difficult to recognize the onset of the lightning catarrh.

A slight chill with pain introduces the scene. There is severe headache in most cases, in some simply a sense of weight, increasing to pain when the head is dependent or when the patient coughs. The whole head may be affected or the pain limited to the supraorbital region, the root of the nose and the eyes. The back of the neck often aches so that the head is drawn back to relieve the pain. We have never found the neck rigid. This pain extends along the spine into the hypochondria and into the extremities. Rarely have we, in the outset of the disease, observed pain in the articulations; later these may be involved as in rheumatism, but without swelling or heat. If there is a square inch of surface which does not ache, we failed to observe it in our own case. There is great distress in the chest, and the abdomen is tender upon pressure. This last symptom is notable in cases with severe diarrhœa.

*Tessier, of Lyons, recently reported the result and investigations made by himself, Roux and Pitton, upon the micro organism found in the blood and urine of divers patients sick of grippe during the year at the Hotel Dieu of Lyons. The micro-organism was studied in different culture mediums (gelatine agar and potato). The characters are very interesting. It is an encapsulated diplo-bacillus reproduced by sporulation, best seen in the potato culture. Its pathogenetic properties have been studied by intra-venous inoculation in the animal.

Finally it has close affinity with the micro organism which Jolles of Vienna, Scifur, of Wurtzburg, and Kirschner, of Berlin, have supposed to be the generator of grippe influenza. It has a certain resemblance to that which has been found in the water of the Moscowa reported by Tessier.

All these considerations lead him to attribute an important pathogenetic influence to this organism.

Early in the disease there is a redness of the eyes with tearfulness. The nasal passages, pharynx and larynx become dry, red and swollen, obstructing the respiration. A constant and exhausting cough comes on which is peculiar to this disease, worse at night than during the day. The patient sinks under the cough and dreads the paroxysms because they increase the pain and exhaustion. The expectoration, in the beginning scanty, becomes at length abundant, airless and of a greenish or ashy gray color. Enormous quantities are sometimes expectorated by patients previously free from bronchial catarrh. Indeed, so large a quantity is sometimes thrown out in the early days of the disease as to make us suspect the patient is bringing up the contents of a freshly opened cavity. The breathing is increased in frequency, and is often laborious even in uncomplicated cases. There are exhausting sweats.

In addition to these symptoms there is profound depression of the physical forces. We have seen patients who were scarcely able to turn themselves in bed on the third day of the disease. This weakness continues for a long time and is the most striking characteristic of the influenza. This muscular weakness is most marked in the lower extremities. We have under observation patients who have not entirely recovered from the attacks of one year ago.

There is fever which lasts about four days ordinarily. Its intensity bears no relation to the sufferings of the patient in uncomplicated cases. Its type is remittent, becoming intermittent at the close. Many patients in our locality have their good and bad days with the regularity of a tertian, for weeks after they are able to leave their beds. We noted one case in which the vesperal rise of temperature was 105°, the morning temperature being 102.5°. Most of the cases never rose above 102.5°.

The pulse was not greatly accelerated in the majority of the cases, but there were some cases in which the

disease produced a remarkable impression. The skin is often covered with a universal blush and the *tache cérébrale* is easily produced in the cases, with intense pains in the head and neck.

A young man, 15, was seized with severe pains in the neck and back of his head. His pulse was 40 per minute for three days and his respiration 12 per minute; at the end of that time it rose to the normal and the patient recovered. A reliable professional brother told me he had seen similar cases and variations of 50 beats per minute in a few hours. Increased inhibition of the heart has been often observed by others.*

In elderly persons the heart showed great weakness, with cyanosis and sweatings, and the tendency to death in all fatal cases was through the failure of the heart.

We have observed œdema of the ankles in some cases during a protracted convalescence, and excitable hearts with tumultuous action after exercise were common. There was anorexia in all cases, some nausea early, occasionally vomiting; not infrequently diarrhœa with severe abdominal pains ushered in the attack. This lasted twenty-four hours or more, when it was followed by the head and respiratory symptoms. In nearly all of these cases there was jaundice before the close of the attack.

* I have recently seen a man aged sixty, who up to the time of his seizure with influenza was robust. He had no bad habits, and had not been sick for years. He was taken sick three weeks ago. He suffered greatly from pains in the chest with oppression in breathing. His temperature rose to 105°. There were no special cardiac symptoms. At the end of ten days he returned to his work, which he continued two days, when he had a relapse.

The pain in the chest continued and he was in agony except when under opiates for two days, when the pain subsided—I then saw him. He was sweating, the skin cool, respiration easy, but his pulse was feeble, irregular, beating from 30 to 40 per minute. I have felt the same kind of pulse in the case of fatty heart. Did the heart muscles degenerate in this case as in infectious fevers, or was the blow solely upon the vagus? Under the use of atropine, ammonia and strychnia he improved. He subsequently became maniacal and violent. After a week he became quiet, and is now improving.

No special symptoms involving the urinary organs were observed. One case had dysuria for two days.

We are informed that for several weeks horses have been showing symptoms, which the veterinarians call grippe.

The eyes are red, so also the mucous membranes of the nose; cough, fever, stiffness on movement, profuse sweatings are present. There are not in a few cases fatal lung complications.

Dr. Norman Gay invited me to see patients who were sick of grippe at the Asylum for the Feeble Minded, near Columbus. There were coated tongues, on the teeth and lips sordes, and in all there was diarrhœa. One of the patients, who left the institution when she became sick, died three weeks later and the case was reported to have been one of typhoid fever. I examined twenty patients during the visit with reference to eruption, iliac tenderness and enlarged spleen but found neither, although all these cases had been suffering from diarrhœa. I feel convinced there was no typhoid poisoning present. Dr. Gay made a curious observation. All the patients sick at the institution with grippe slept in rooms having an eastern or northern exposure. The rooms having a southern or western exposure, although they were occupied as dormitories in an epidemic of several weeks, furnished no cases. The same restriction as to locality of cases in the rooms having a similar exposure in the Central Asylum for the Insane is also reported.

To the east and north there is a wide extent of low ground, but it is well drained and cultivated, so that it seems impossible to attribute the localization of the cases to this circumstance.

Complications were common. Those involving the nervous system were most numerous. There was mild delirium in many cases. Mental confusion with gloomy anticipations were present in several cases. Instances are reported in which this state of affairs has resulted in sui-

cide and confirmed insanity. Happily we have seen none of the latter cases.

In the epidemic at Lyons, in 1655, many persons are said to have died insane.

Dr. A. B. Richardson assures me that he has recently seen cases of insanity, the sequelæ of grippe.

Muscular spasms are said by the authorities to have occurred in other epidemics. My own feet and limbs were thus affected.

One of my patients, while convalescing, told me she was impelled to laugh or cry without cause.

We have seen croupous pneumonia in the middle-aged, and broncho-pneumonia in the aged, and in children. We have had two deaths from croupous pneumonia and three from broncho-pneumonia among the aged, and have seen several, in consultation, who have died. They have perished from heart failure. Heart failure seemed to us to be the most common if not the sole cause of death in the fatal cases, the lung choking being secondary.

Jaundice has been observed as before mentioned.

Suppuration of the ear with perforation of the tympanum has occurred in two cases. Suppuration of the parotid gland in one case, herpes labialis has been a common affair, and in one case herpes zoster has occurred. In not a few cases the pharyngeal symptoms have been severe, with isolated white patches on the tonsils with much accompanying œdema.

We can report one case in which there was albuminuria which lasted many months after the influenza. Whether this was a sequel or not we are unable to say; the condition was not recognized till the patient was convalescing.

In two instances we have observed abortions which we think may be fairly attributed to the baleful influence of the grippe. As a sequel we have seen articular rheumatism three times.

We have seen several cases in which acute tuberculosis

has followed grippe; whether it was latent at the time of the attack, we can not tell, as we had no opportunity to examine them previously.

Relapses are common, and second and third attacks not infrequent. Such has been our personal experience with this disease.

Treatment.—When the pain is severe at the outset of the disease, we give morphine and atropine hypodermatically in doses sufficient to relieve the pain, or full doses of Dover's powder by the mouth. To children we give small doses of veratrum, or aconite and morphine. In less severe cases we give phenacetine, especially when the fever is active. Antipyrine and acetanilid we abandoned early, because of their depressing action on the heart. Last year we saw the case of a physician with hydro-thorax and general dropsy following grippe, which was hastened to a fatal termination we believe by the excessive use of antipyrine.

For respiratory trouble, carbonate of ammonia in full doses combined with strychnine has been our favorite treatment; later in the disease for the residual bronchitis we give terebene, apomorphia and cocillana. For the debility and overpowering sense of weakness we have found nothing which has seemed to relieve the patient as much as strong wines or whisky.

Later arsenic, zinc and iron with cod liver oil seemed to be of service. We derived no apparent benefit from quinine, even when the patients have their alternate good and bad days.

Topical measures were used as demanded for throat and lungs, with hot poultices and hot water bottles to relieve local distress.

Milk, eggs, custards, soups, are demanded throughout the disease.

DISCUSSION.

Dr. G. Goodhue:

The term influenza seems to be a very broad one. Under its head several diseases may be classed, as different

almost as white from black. I was curious to know what symptoms the doctor would give of influenza. In my experience the influenza of 1890 and the influenza of 1891 are so distinct that they might almost be called different diseases. In '90 it was my experience that I was summoned from my office, and urged to come with the utmost speed. Although the disease was known to be almost local, still, when it struck a person, it not only pained him, but it struck him with fear. He felt that it was his last day. His physician must come and come at once; if he could not come, some other one must be obtained. Fever, high headache, almost intolerable. It ran its course, usually, in a comparatively short time. While in the influenza of '91, so far as my experience is concerned, the symptoms were considerably different. Fever not so high; headache still, often altogether absent; we are not called in in a hurry, but are asked to come at our earliest convenience, and often, on account of the symptoms being much less prominent than the year before, it was considered a mild affair; our patients would undergo risks that were unwarrantable, and, as a result, many lives were lost. These two diseases when compared are different altogether in their symptoms; the symptoms of one being very severe, the symptoms of the other, comparatively mild. It is my experience that the influenza of '91 in its permanent effect has been greater for evil than that of '90. I question whether they are the same disease—whether for that of '90 we should employ the term grippe.

Dr. E. Sinnett:

It strikes me, from the remarks of the gentleman who has just taken his seat, that the condition he describes must be owing to the severity of the symptoms of the cases in the locality in which he resides. I can't but believe that the influenza of '90 and '91 are identical, although I will agree with him that these high symptoms of prostration probably are more manifest than they were a year ago, in many cases. There have been in our locality several distinct times and periods when this disease was aggravated largely. During this winter past I have been located on a hill some two hundred feet above the surrounding country. It was very noticeable that until nine o'clock of the day, and for three or four days preceding an acute increase of the influenza, that the whole

country was densely covered with fog, and damp, heavy atmosphere, so that there was no house visible for miles. Whether that corresponds with the condition observed at the asylum, which the doctor reports, I cannot say. However, on several occasions I noticed this condition of affairs in our locality. I think the symptoms have been described very fairly. There is a difference in them. In our section we have had a great deal of hemorrhage—a great deal of bloody sputa, bronchial discharge; immense in some cases; but as a rule recovery ensues, after a tedious course of prostration.

I agree fully with the doctor in the disuse of quinine in these attacks. Many symptoms seem to indicate that the use of quinine will be advisable, but certainly in my cases it has been of no use, but I think it does injury. It does injury in this, if nothing else—it is used where other remedies of more value could be substituted.

Dr. N. Gay:

I had charge of an institution part of the time during the epidemic this winter. I am not going to discuss this question, but simply cite some instances connected therewith. If my theory is right, I believe we may avoid a good deal of this trouble by promptly closing the east windows. You think this is very strange; but that seems to be the history here. The building has a front. It runs north and south, and has a front of about eighty feet, in front of those, for east windows. Then you have a recess back, and then another front east, and then another recess back, and then another front east. The south is arranged in the same way, first the front, then the recess, then the east front, and then another recess, and another east front. The dormitories, where the patients are, are on the east side of the north and south hall. The first attack seemed to be in the front building, and this attack was with diarrhœa, like cholera morbus. It commenced with the girls on the north end—probably two hundred cases of diarrhœa; and not a single case occurred among the boys in the south end of the building. In the dormitories on the east side of the hall of the main building, running north, the east dormitory had ten patients, and the west dormitory to the west side of the hall had one, with this diarrhœa.

The first notice that we would have of the sickness of

those children, they would commence to vomit—vomit when they went to their meals. We would find them with a very dry, hard tongue; and within two days we would have a diarrhœa. The diarrhœa was almost impossible to check. Nothing in the world would stay upon the stomach except lime water and milk, on which we had to feed the children sometimes three weeks; anything else they would immediately vomit up.

In the south end of the building we had some of those attacks, but no diarrhœa—not a single case of diarrhœa in the south end of the building, among the boys.

The east wind coming in would blow directly into those buildings. The officers and teachers of the institution, who roomed in those north and east rooms, a large number of them suffered with this disease; ten or fifteen of them were on the sick list at one time; and they all suffered with a diarrhœa. The young men who slept in the boys' department, in the south end, none of them were attacked. The worst attacks we experienced were among those who slept where the east wind went through the room. The attacks occurred from twenty-four to thirty-six hours after we had these cold east winds. It apparently did not affect us very much in the house, because the house was warmed by steam all through, and one department is just about the same temperature as the other. There didn't seem to be any excuse for the sickness, as I could see—certainly no local trouble. If there had been a local trouble, it seems to me that it would have attacked with a south wind as much as with a north wind.

If the theory is right, we certainly might do a great deal to prevent the disease in windy times. It is a terrible disease on a great many; it does not end with the first getting up, by a great deal.

Dr. W. B. Davis:

The Actuaries' Life Insurance Co., of America, have prepared some statistics of the mortality of this disease, as manifest by the returns of the several companies of our country. It has increased the mortality from three to five per cent. What is true of the insurance companies is true of the population at large. I was in New York quite recently, and my attention was called to the fact as to the influence of this grip upon the population even where they were not aware of it. That

has been manifest among the class of persons, apparently healthy, who apply for insurance. These parties were examined at the home office by experts, and consequently there was no room for error; and it was found from the statistics of those companies that there was an increase of 100 to 600 per cent. in cases of albuminuria, where the parties were not aware of it, and did not know that they were in poor health. The statistics as prepared by these experts for the year 1890, or rather I should say of the mortality for the year 1890, show 75 per cent. of the cases of grip proved fatal through lung diseases—by pneumonia, bronchitis, etc. I was struck with the similarity of these statistics with those of the hospital at Stockholm, where they have had a very extended epidemic, to find the results as to mortality were just about the same.

Dr. H. S. Herrick.

We are treating all the diseases known to man under the one name of influenza, because it prevails at this time. It is not an epidemic. That is not a happy term at this time. I would say "epidemic influence on the community manifested in these different ways." I think the term influence is the better one, although the paper was a very commendable one, but the term is an unfortunate one. The discussion in the paper is a purely scientific one. It involves so much research into the pathological condition, and the epidemic influence affecting the organisms, that a better term should be employed.

Dr. A. B. Richardson:

One year ago I had charge of an asylum for the insane. Of course I can't very well compare the experiences of the two years, because my situation was so different. At that time I was in charge of an institution located at Athens, Ohio, the building running practically east and west almost; the female department on the west side and the male department on the east side. There were from sixty to seventy-five female patients attacked with the grip symptoms, with coryza following the ordinary symptoms. On the male side we had a very few cases. I think we had one death, of a very old patient, nearly eighty, from pneumonia. This year I have been located in Cincinnati, pursuing a special practice chiefly, and it has brought me in contact with the sequelæ of the

grip rather than the grip symptoms themselves. I have been particularly struck with the influence it has upon the nervous system. I have been called in consultation at least in three cases of insanity following the grip, in which it seems to me that the influenza, or the influence of the epidemic, was very pronounced as a factor in the case.

Dr. Kinsman:

With reference to Dr. Goodhue's remarks, I would say, the influenza may spend its force on the respiratory and nervous systems or upon the digestive organs. I have mentioned this in my paper.

Last year, in the greatest number of cases the force of the disease was spent on the nervous system. The sufferings of the patient were worse, but the sequelæ were not so severe as during this year. This year the respiratory organs and the digestive apparatus have been oftener involved.

There has been great confusion of terms, and doubtless many cases of sickness occurred, and were called influenza, which were not influenza at all. However, there can be no doubt that influenza has had a decided prevalence this year.

CASE OF FRACTURE DISLOCATION OF THE SPINE, WITH OPERATION.

BY A. W. RIDENOUR, M. D., MASSILLON.

In view of the rarity of these cases with the almost invariable result, I have thought proper to report a case that marks a new era, at least in my practice.

We will remember at the start the severe trauma the cord underwent; the bones crushed; muscles and ligaments torn loose.

December 11, 1890, Martin Nye, aged 28, laborer, while driving through a shed on a load of lumber, was caught between an overhead beam in such a manner as to crush the center of his back forward at an acute angle, frightfully lacerating and crushing both hard and soft parts in that region.

Within half an hour after the accident found the patient pulseless, skin cold and clammy, respiration sighing, very restless, jactitation, suffering frightful pain in back, sensation and motion absent from injury down, knee jerk absent, reflex absent.

On inspection at seat of injury found depression of fully one inch corresponding to seventh dorsal vertebra, with absence of spinous process of eighth dorsal. This at once led me to inform the patient and friends of the exceeding gravity of the case, and that the only hope of life lay in a speedy operation. Consent was readily given and the operation was at once carried out.

With the usual and necessary antiseptic preliminaries an incision about twelve inches long was made, commenc-

ing at the third and terminating at the ninth dorsal, separating muscles from either side and exposing all bony roof of cord from the sixth to and including the ninth dorsal. The muscles were crushed, soft, pulpy and mangled beyond recognition in region of injury. The lamina of seventh dorsal broken and separated from the body; transverse process either side of seventh fractured, with facet for ribs torn completely off; ends of ribs stuck up in wound like two fists; the body of seventh vertebra was dislocated forward fully one inch, or nearly slipping out in front. Spinous process of seventh vertebra was split and crushed.

The spinous process of the eighth dorsal was not only split but actually inverted, penetrating the cord, crushing it; the lamina of eighth fractured; the body of eighth fractured but not dislocated. The membranes were punctured and badly lacerated at different points, with free hemorrhage in the arachnoid space. The cord was crushed at juncture of seventh with eighth by the inverted spinous process of eighth; the cord was carried forward with the dislocated seventh vertebra; compression at juncture with sixth and eighth. Altogether I can not conceive of a more frightful injury unless we might include cutting off the cord, and immediate death of patient. Did not use trephine, did not have to. Do not think it safe to trephine the spine. The bones being soft can be separated readily with safer bone instruments.

Removed all bony roof from middle of ninth dorsal to and including middle of sixth dorsal vertebra; passing my finger between the body of seventh dorsal elevated it into position; smoothed all rough or sharp bony prominences; removed all clots, stopped all hemorrhage, replaced dislocated ribs as well as I could without any articular facets, and placed strands of catgut along bottom of wound in such a manner that the cord would not come in contact with them, or in fact with anything, (requiring some care here in the disposal of remaining

bones and overlying muscles). Sutured muscles carefully on either side, sutured skin, applied dry dressing.

The after treatment of this case was uneventful. The sensation returned at once in both lower extremities; motion perceptible in recti muscles at end of fourth day. Compelled to use catheter to pass urine until the seventh day, when it passed normally. No control over sphincters ani until the end of first week, when it was restored. Knee jerk restored at end of second week; could raise limbs from bed at end of fifth week; can walk with crutches at end of three months. Applied plaster of paris jacket for support at end of second week.

The wound healed without any pus and with three dressings primarily in one week. No fever at any time. Urine did not present any albumen or sugar; no casts or abnormal deposits, sp. gr. 1,020.

Patient suffered no pain after operation; no atrophy of muscles of lower limbs since operation; joints not thickened. Patient walks about with aid of cane June 5, 1891.

The patient is under care of Dr. Marchand, of Canton, and is at the Stark County Infirmary. The doctor reports the patient as rapidly recovering his bodily vigor, can get around readily and that the plaster was removed during April. There is no deformity at all. I would have wished the plaster had remained on longer, but it seems no ill effects followed its removal. I was kindly assisted in the operation by Drs. Miller and Reed, of Massillon.

The only plea I have to make in these cases where the diagnosis is made, is for an immediate, thorough and careful operation; a few hours delay in my opinion is sufficient to render the operation futile, and I have no hesitation in placing the cause of failure in so many of these operations to delay.

Some eight years ago I reported a case to this society where I had made the operation three years after injury with the result of complete failure, patient dying two weeks after, of acute albuminuria.

But in doing this work the utmost care is required not only in removing all bony or other foreign material from the vicinity of the cord, but that no possible chance for subsequent hemorrhage or pressure from exterior cause can occur, as even the pressure of superimposed muscle will be sufficient to deprive the cord of life; or a small shaving of bone will cause inflammatory softening and death. Slight hemorrhage will cause a clot that will act more rapidly than in the brain in causing death; but the operation can be done and should be made safe by care and without hurry, and I will repeat, without delay. No patient should be allowed to die without an attempt by an operation to save him.

SOME FACTS EVERY PRACTITIONER OUGHT TO KNOW ABOUT SQUINT.

BY ALBERT RUFUS BAKER, M. D., CLEVELAND.

Professor of Ophthalmology and Otology, and Clinical Professor of Diseases of the Eye, Ear and Throat in the Medical Department of Wooster University. Consulting Oculist and Aurist to the University, Women's and Children's, and City Hospitals.

I meet doctors all over the country who do not know the difference between a hypermetropic and a myopic astigmatism—practitioners who do not possess an ophthalmoscope or trial lenses, and who would not know what to do with them if they had them, who say, "I make no pretensions to treat eye diseases, except those of the most trivial character, such as granular lids and *simple operation to cure strabismus.*"

When I answer such remarks by saying that there are few subjects in the entire field of ophthalmology requiring so much judgment and such a wide knowledge of the anatomy and physiology of the eye, and so thorough a mastery of the intricate subjects of refraction and accommodation, as the treatment of squint, I generally lose, in the esteem of doctors of this class, the reputation I may have as an oculist. Two or three unsuccessful operations often serve to modify this unfavorable opinion very materially. As an illustration of the frequency of the failure of operations for squint, in the hands of surgeons not having a thorough knowledge of refraction and accommodation, and the optical principles involved in the treatment of these cases, I may quote the following statistics of consecutive cases, as revealed from my case-book in private practice:

Advancement of the internal rectus has been per formed

thirty-two times, and thirteen of these were cases in which the surgeons had divided the internal recti too freely, and the case of simple convergent squint was changed into a marked divergent squint, leaving the patient in a much worse condition than if no operation had been performed. Several of these cases occurred at the hands of surgeons of undoubted skill. During the same period of time, eighty-four cases of convergent squint have been operated upon, and twenty-seven of these cases had been operated upon by other practitioners unsuccessfully. Some of them had been subjected to as many as eight to ten operations.

One of the facts every practitioner should know about squint is, that no surgeon should perform an operation for cross-eyes who is not competent and prepared to make a careful scientific examination of the patient's refraction, and prescribe spectacles accordingly. There are thousands of cases of squint scattered all over the country who have been operated upon unsuccessfully, or who have knowledge of unsuccessful operations, and who go through life with this deformity uncorrected, because nearly every young graduate of medicine who has seen a few operations performed in the clinical amphitheatre, when a student, thinks himself competent to make the operation, and experiments on the first case he meets. It is not surprising that the operation has fallen into disrepute in the minds of the laity.

Another fact every practitioner ought to know about squint is, that the object to be gained by treatment is not only a cosmetic one, but to establish and preserve binocular vision.

The person suffering from squint sooner or later becomes, to all intents and purposes, blind in one eye. He has a blind side. He, to a large measure, has lost the "perspective sense," and cannot judge of distance.

Is the amblyopia of the squinting eye the cause or the effect of the squint? The weight of opinion seems to be

that the theory of Donders is in the main true. The squint depends upon hypermetropia and the interdependence of accommodation and convergence; the amblyopia is the result of disuse, psychical exclusion, or suppression. The leader of the opposition is Schweiger, of Berlin. He thinks the amblyopia is monocular and congenital, or that it precedes and is one of the—if not the sole—causes of the squint. Cuignet sadly misunderstands or misrepresents Donders' theory, and brings forward the strange theory that the child conceals the squinting eye beneath the internal angle and under the shadows of the nose and brow because of photophobia. A "reflex" exercised by the defective eye is juggled with.

The strongest argument brought against the views of Donders is, that all children are hypermetropic, and, consequently, the connection between hypermetropia and squint is not so evident as heretofore supposed. There are several answers to this observation. There is undoubtedly a difference in individuals in this respect. Some are able to exercise greater accommodation with less convergence than others. They have more "play" in this respect and are able to resist the tendency to squint. It has also been shown that the average amount of hypermetropia is much greater in squinting than in non-squinting children. Mr. Frost finds in his own practice that hypermetropia under 1 D., which is common in children, is seldom associated with squint. A majority of his strabismus cases had a refraction of over 2 or 3 D. of hypermetropia, and over 10 per cent. had H. of over 6 D.

Mr. Snell's cases of squint averaged over 4.25 D. of hypermetropia.

Landolt quotes the following conclusive experiment as proving that hypermetropia has a tendency to produce a squint: "If we possess binocular vision, let us fix a near object, cover one eye, the left for instance, and place a concave glass before the right. This eye will not change its direction, but will continue to see clearly, but the

effort of accommodation, which it is forced to make in order to neutralize the negative glass, imposes itself at the same time on the other eye, and provokes in the latter a converging strabismus of a degree corresponding to the power of the concave glass. The existence of this strabismus may be easily established objectively, and manifests itself subjectively by a homonymous diplopia at the moment when the diaphragm is removed from in front of the left."

It is certainly a matter of almost every day experience with practical ophthalmic surgeons, that the correction of the ametropia is frequently all that is necessary to cure the squint; the moment the glasses are laid aside, it reappears.

Another fact every practitioner ought to know about squint is, that most cases can be cured without an operation. Of one hundred and ninety-eight consecutive cases of convergent strabismus occurring in my private practice, one hundred and six, or more than one-half of the entire number, were cured without an operation. I say they were *cured* advisedly, because when I commenced using spectacles for the cure of strabismus, I found that when I charged fifty dollars for an operation for strabismus, patients paid it willingly, but when I charged fifty dollars for fitting spectacles, even though it required infinitely more skill and time than was required for the operation, strenuous objection was made to paying so large a fee; so that to keep my patients, and at the same time secure a moderate fee, I fell into the habit of charging a stipulated amount, and if the spectacles did not effect a cure, I agreed to make the operation without additional cost to patient. In this way, with very few exceptions, I have been enabled to follow my cases to complete recovery. A review of these cases has suggested a number of interesting questions. We may have determined accurately the refractive condition of each eye, and yet be at a loss to know what lenses to prescribe. The glasses that are

theoretically correct may be absolutely refused by the patient. The number of changes made in these prescriptions is a painful attestation of this fact. Must the whole amount of the error of refraction be neutralized? If there is a difference in the refractive condition of the two eyes, shall both eyes be corrected fully? If one only, what is to be done with the other? Must the spectacles be worn all the time? When, and how long should atropia be used? When will two pairs of spectacles be needed? What are the indications for decentering the lenses, and what for prisms? The question of personal appearance also has to be taken in consideration, and will have a decided influence in rendering a verdict as to what style of frames shall be worn—eye-glasses or spectacles, which? These are problems that require the best judgment, and try the skill of the oculist.

Every case presents a problem to be solved. As a general rule, the nearer a full correction of the error of refraction is attained the better. In order to do this accurately the accommodation should be paralyzed with atropia in every case. Spectacles should always be given in preference to eye-glasses.

Treatment based upon these facts, viz., that the squint is due to the error of refraction, that binocular vision can be restored, and that squint can be cured without an operation has met with the most gratifying results. If cases are seen early and treated carefully it may be a question whether it is ever necessary to make an operation.

Mothers are as a rule accurate observers of their children's ailments, and I am constrained to put more confidence than formerly in the statements frequently made by mothers, such as the following: "My child's eyes were not crossed until he had an attack of the measles, or some other illness, or had a slight injury of one eye, and it was tied up for a few days; when the bandage was taken off the eyes were crossed; or he imitated a cross-eyed playmate; or he amused his little sister by making

eyes." These children might have gone on through life without squinting, but for the trifling incident that for a short time unbalanced the natural tendency there is in all cases to establish and preserve binocular vision, but owing to the excessive nervous stimuli that are sent to the internal recti in all cases of hypermetropia the squint soon becomes permanent. The practical application of this observation is that many cases of squint may be prevented by a little timely advice to the mother.

While every case of hypermetropia does not squint, every case of squint is hypermetropic. The statement, that once hypermetropic always hypermetropic, is not true, as will be attested by any one who has kept cases under observation year after year. I have notes of a case, a boy six years of age, brought to me about seven years ago, with convergent strabismus; he had two dioptries of hypermetropia. I gave him a pair of spectacles correcting the error of refraction fully. The eyes soon became straight; about two years afterward he returned and said he could not see the letters on the blackboard at school with his spectacles. I found that he had only one D. of H., and changed his spectacles accordingly. In about another year he came back complaining about his spectacles, and said that he could see better without them. I found that his refraction was normal, and ordered him to stop using spectacles. About a year ago he returned, complaining that he could not see the blackboard, and I found that he was becoming myopic, and at the present time he has about 2 D. of myopia. Here we have a typical case of squint with 2 D. of hypermetropia changing to emmetropia and then to 2 D. of myopia in seven years. It is quite possible, that even though this boy had not been fitted with spectacles as the eyes changed to normal, the squint would gradually become less and finally disappear altogether. I have seen a number of such cases, but they are exceptions. Even though the refraction change to normal or even to myopia, the squint almost

always remains, because the one eye usually becomes blind before the refraction has changed to normal, and then no effort is made to establish binocular vision.

This observation has a very decided bearing upon the treatment of these cases. If we can preserve the sight of the eye until such time as the eyeball changes its form and becomes emmetropic, or until the child is old enough to wear spectacles (which serves the same purpose as nature often accomplishes in changing hypermetropic eyes into normal ones), we treat our cases in accordance with scientific principles and secure perfect results. Fortunately this can be done very easily; simply give the mother a solution of atropia, instruct her to watch the child; if it commences using one *eye all the time*, put a drop of atropia in the fixing eye. This paralyzes the accommodation and compels the child to use the other eye. Repeat the atropia once or twice a week until the child begins to use the other eye. It may be necessary after a time to use the atropia in the other eye as it often becomes the fixing eye. As long as the child sometimes uses the right eye and sometimes the left, there need be no fear of the child becoming blind in one eye, and there is no necessity for using the atropia in this manner, although much benefit may be gained by using it in both eyes occasionally for a short time. Girls will usually wear spectacles satisfactorily at three and a half, and boys at four and a half years of age.

In treating cases of squint I have usually been governed by the following rules:

1. If the squint is alternating and the vision fairly equal in both eyes, it is seldom necessary to operate. A full correction of the ametropia will usually result in cure of the squint.

2. If the squint is fixed in one eye, but the vision of the squinting eye good, the same rule should be observed, excepting that atropia should be instilled into the work-

ing eye occasionally and possibly a patch kept over it, and orthopedic exercise indulged in as described by Landolt.

3. If the squint be fixed in one eye and the sight very defective, and no improvement after patient trial with lenses and covering good eye, only a cosmetic result can be attained. The operation should be performed any time after the sixth year.

4. If the squint be fixed in one eye and the vision of this eye is slightly defective, it is possibly undergoing deterioration from disuse, and should be carefully exercised, watched and tested. If the deterioration of vision appears to be increasing, an operation should be performed at once.

STRABISMUS CONVERGENS.

BY C. W. TANGEMAN, M. D., CINCINNATI.

Assistant to Prof. Seely's Eye Clinic; Eye Surgeon to the German Protestant Hospital, Cincinnati.

By strabismus we mean a disturbance of the optic axis of one or both the eyes. The deviation inward is by far the most frequent deformity, while it may occur in any direction. It is the deviation inward that we want to consider. The eyeball is moved by a system of muscles very delicately arranged, and relatively at least well balanced. These muscles are so arranged that even when at rest they have a tendency to draw the eye backward into the socket. Should the relative strength of one or the other muscle now be interfered with, the natural tendency would be a preponderance of power in the opposite direction or a deviation of the eyeball toward the opposite side of the muscle affected even when at rest, but when the muscles are called upon to act for near fixation the deformity is exaggerated many fold. Such a deformity could be easily recognized even though the associate as well as the separate movements of the eyeball would not be interfered with.

In the subject before us, convergent squint, there is no disturbance of the strength of any of the ocular muscles. If each eye be tested separately we will find that the squinting eye can rotate outward just as readily as the opposite fellow. To satisfactorily give the cause of this deformity may be difficult, but we claim it to be dependent in a great measure at least upon the refractive condition of the individual's eyes. The degree of convergence

of the visual axis depends on accommodation, or the effort that the patient must make to see. The greater the degree of hypermetropia the greater the effort to see distinctly.

Now, in these cases where there is such a constant and intense effort to see distinctly at the patient's near point, there is danger that the accuracy of the visual angle be disturbed, and the two eyes no longer look at the same point of a given object at the same time; and the result is double images, or each eye sees its own image of an object. Such a condition of things cannot continue for any great length of time, since the confusion to the patient causes him to get rid of one, or suppress one image. The squinting eye is no longer called upon to assist in the visual act; it becomes amblyopic, or it turns sufficiently far inward that the image from it is so far away from the image that the patient is regarding, that he is not annoyed by it, and finally it is suppressed entirely. It is natural to see but one image of an object looked at. As soon as the visual angle is disturbed sufficiently to cause diplopia, nature makes an effort at a rescue, rendering the retina of the squinting eye insensible to impressions, or deviates the axis so far to one side that the image is no longer in the visual field. The diagnosis in these cases is not difficult, and the whole question of interest hinges itself on the method of correction or restoration.

The correction of squinting eyes formerly was considered a surgical subject, and a great many tenotomies of the recti muscles were made by the surgeon, regardless of the cause of the deviation. Such careless procedure bore its fruit, and to-day the operation is an exceedingly rare one with many surgeons, met with so seldom that material for instruction is often not to be had when wanted.

Hansen apparently struck the key note when he asserted that strabismus is due to an abnormal range of accommodation. We all make an effort at binocular vision. In

the normal eye, convergence fulfills the demand of binocular vision, while in squint we disregard binocular vision and satisfy accommodation. This would make convergent strabismus depend on the relation between accommodation and convergence. For example, our patient is hypermetropic, he must make an effort to see an object at a distance, and a much greater effort when he regards near objects. This great amount of effort brings about too much convergence; the visual axis is disturbed and the patient sees double. This theory, while it has its exceptions, can be verified daily in the clinic. The large majority of these cases occur in children ranging in age from two to six years, or the time of life when objects are first regarded closely.

In the child, where it requires but little disturbance to bring about this condition, we find strabismus present about the time he regards his toys closely, while in another child it may not show itself until he has attended school for some time. There are as many squinters now as in times past, and it is the business of the oculist to give relief. Formerly, any and every deviation of the visual axis was immediately corrected by operative interference. In the child the line was drawn only where it was too young to tolerate an anæsthetic, and in the adult the final immobility of the eye.

A correction of the deformity then as now was the aim, yet this operation has become less and less frequent, until we can scarcely find material to demonstrate the technique of a tenotomy to a class of students. You very naturally ask, why is this, and are the reasons good ones? The first observation made was that these children complain of pain in the eyes when they look at their toys, alphabet, or apply themselves closely to their books. Next it was found that these children are always hypermetropic, or have some refractive trouble which compels them to make an effort to see distinctly. Glasses adjusted according to the existing conditions nearly always relieve

the above detailed symptoms. Where there is a disturbance of the visual axis this symptom in nearly all cases disappears. If the child loses the glasses or they are not worn for some time the tendency to squint when looking at near objects reappears at once.

Does this argue anything when speaking of the cause of strabismus? Now the last and most forcible argument against operative procedure in children, yes infants, is that in a large number of these cases there is subsequently at some period of life, usually at the age of about thirty, a distinct outward deviation of the operated eye setting in. The development of the face, the broadening of the base of the visual axis, perhaps, has a tendency to overcome the inclination to convergent squint as the child grows, since it is never known to develop only in childhood. It is argued that all children are hypermetropic at birth, and as they develop, their eyes become normal. If this be true, then the development of the eye naturally is not sufficient to overcome the tendency to squint in these children where the visual axis is disturbed so early in life. The symptoms of hypermetropia (a condition where the eyeball is too short from before back) are quite distinct in the adult, and if the child is old enough he manifests the symptoms equally well. He complains of pain about his eyes, sensitiveness to light, gets tired easily of his picture books, and wishes to hide his face somewhere where it is dark. The ophthalmoscope shows hypermetropia to be present, and a little later there is a distinct deviation of the axis of one of the eyes noticeable.

Relieve the hypermetropia, or, in other words, relieve the strain or effort that is necessary to see distinctly, and the patient becomes comfortable, and the tendency to squint disappears. What is to be done with the child too young to wear glasses? Quite a number of clinicians still cry operate, but the number of observers is constantly increasing who have found that a tenotomy only relieves a symptom and completely ignores the cause. The plea of

the advocate for the early operation is, "save the vision of the squinting eye," and the question arises, does a tenotomy do it? Not any more than very much more simple methods.

All squinters are shown to have some refractive defect, which, when corrected, relieves more or less completely the defect of the visual axis. Take off the glasses and the squint returns, showing clearly that there must be some connection between hypermetropia and its effects on the eye as a cause of concomitant squint. In the child that is too young to wear glasses we must paralyze accommodation by the regular use of atropine, thereby preventing near vision. This treatment may be continued for months without injury. It recommends itself on account of its simplicity, and because it leaves no bad after-effects. It is a well-known fact that in some patients the tendency to convergence passes away as they grow up, without any treatment of any kind; in others it disappears while using atropine; in others while using glasses. This constitutes the large majority of squinters, and whenever a tenotomy is made, in any of the cases enumerated, it is found to not only be superfluous, but harmful ultimately.

It is the writer's opinion that a tenotomy is permissible only after the refractive condition of the eyes has received the most careful consideration, and glasses have been worn for a year or more. If you carry out these instructions you will see but very few tenotomies. I have seen no less than ten patients during the last year with external deviation caused by a tenotomy of the internal recti muscles during childhood. Some of these patients had never worn glasses; it had never been suggested to them.

I want it plainly understood at this point that I am not saying that the tenotomies were improperly done; in fact the patients all claimed that the result was perfect when first done. Just what the causes are (other than mentioned) that bring about an external deviation twenty

years after section of the internal rectus, we cannot say. It has never before been put into print that such a large number of internal tenotomies eventually result in external deviation, especially if the operation had been done in children and no attention was given to the refractive condition.

Tenotomy without reference to the refractive condition is never a safe procedure. The effect on the disturbed visual axis from glasses is not always so rapid nor so marked as after an operation, but years of observation have proved that the more slow method is the more satisfactory in the end. In the child where glasses cannot be worn, a solution of atropine must be used regularly (once every five or six days). This does away with near vision and any necessity for convergence. Such treatment may be kept up until the tendency to squint is outgrown, or glasses can be worn. If there is not already amblyopia of the squinting eye, atropine will hinder its development just as well as an early operation (the only argument for early operation). In fact, we are not at all certain but that amblyopia is one of the causative factors in this disturbance. It was a great stride forward when tenotomy of the internal rectus was first practiced over the old plan of correcting the deformity by tonics, barks, eye shades, partially opaqued glasses and muscular gymnastics, because few if any cases were cured except such few as correct themselves. But our last advance is more significant, since it is a step in the direction of solving the cause of strabismus, and correcting the deformity, instead of giving our attention only to the correction of a symptom.

DISCUSSION OF DR. BAKER'S AND DR. TANGEMAN'S PAPERS.

Dr. S. L. McCurdy:

I would like to ask Dr. Baker if he would expect a general practitioner to know all that he has said in this paper? We have some of us aspired to knowledge in

this direction, and have made efforts to correct errors of refraction and accommodation, and frequently succeeded in adjusting glasses in special cases of simple myopia or simply hypermetropia with very little difficulty, but in compound myopic astigmatism, or some error of that kind, it is very difficult to do, and I don't know just what practical benefit we could get from having the knowledge which is expressed in this paper, because, unless we have this knowledge at our hand, so that we could successfully adjust glasses, the knowledge would amount to nothing. The only thing we could do would be to send for a professional. We can't succeed in doing anything of this kind. I would say that I have made all the effort possible to adjust glasses. I have a test case and an ophthalmoscope. I have found in some cases that it is the easiest thing to adjust glasses with the utmost exactness, because you can measure the degree of error of refraction and accommodation — measure the degree of astigmatism exactly; but we can't always do that. In some cases I can do it; in many cases I have worked for weeks, and could not succeed in doing anything. The papers of Dr. Tangeman and Dr. Baker, of course, are valuable as knowledge in that direction. We can absorb it, and use it in general practice.

Dr. X. C. Scott:

I had not the pleasure of hearing the last paper, but at the first paper I must say I have been astonished. I must say that the gentleman has had a most extraordinary experience with the advance of the internal recti muscles. He probably has been in the practice longer than I know. To have had thirty-two advancements of the internal rectus is extraordinary. These cases requiring the advancement of the internal rectus are not so very frequent. It is astonishing that the gentleman has had so many, unless he has had an extraordinary experience. Also, that more than one half of these cases can be rectified by means of glasses. It is true that a certain number of cases can be rectified by means of glasses. That is, where the squint depends upon a mild form of hypermetropia, where the squint has not existed for any length of time, and where the internal rectus has not become hypertrophied. In those cases you can rectify by means of glasses. But when you say that out of one hundred and eighty or

more you can rectify one hundred and six, I don't believe it. It is a mistake, unless this gentleman has met with a certain number of cases and has had an experience more extraordinary than that of any oculist that I know of. There are a certain number of cases that can be rectified; that is true, if you get them when they first start, because they depend upon simply a mild form of hypermetropia, but only unless the squint has been of a mild form. When those muscles become hypertrophied, as the arms of a blacksmith become hypertrophied from use, you can't rectify that squint unless you divide the rectus. I speak now of convergent strabismus. Where we have general convergent strabismus accompanied by hypermetropia, this you can't rectify by glasses, 25 per cent.; you can't rectify 60 per cent. nor 66 per cent.

Dr. Tangeman:

If any person understood me or my paper as being intended for the specialist, I beg to say that that was not the intention. I intended in my paper to make it just as simple as possible, in order that the general practitioners might understand these cases as they come to them, whether they are competent to take care of them or not. I, for one, certainly am not opposed to every individual doing what he is able to do; if he is able to do it, do it. I say this, although I limit my practice to the eye. It was not any proscribed or prescribed rule, as to how he should proceed in the treatment of children or in the treatment of infants—the small infants that are beginning to manifest signs of hypermetropia, and are beginning to manifest signs of strabismus. That was the class of patients concerning which I suggested the treatment. The laity, as a rule, whatever other ideas they may have about the causes of squint do not know how easily many of these cases, with this condition, may be relieved. They now believe that the patient will outgrow it. I presume that one out of twenty-five will have that idea. In that class of cases where the eye becomes extremely turned in, I suggested the use of atropine, not that you can cure your patients in a large number of cases. If you are not competent or prepared to put on the glasses after that, do what you see fit to do.

With regard to the number of cases cured by glasses, I do not think that I am able to state any statistics on

that subject. I would say that Dr. Baker, in curing so large a number as that, by glasses, has been very successful. I did not know that so many people of Cleveland were cross-eyed. I have been up there a few times, and I did not see many cross-eyed people up there. That is a large number of cases to show for the length of time that the doctor has been practicing. I do not think we have that number in Cincinnati; and we do not get fifty dollars for each case.

I would say that I would not operate on any case until the child or individual had worn glasses for at least a year. I have had two patients in my office within the last week or two with the eye turned.

"What was done with that eye?"

"Dr. So-and-so operated when I was a child."

"Did he leave it in that way?"

"No: it was absolutely good."

We should not operate until we are satisfied ourselves that we have gotten all the benefit from glasses we can get. "You operate?" somebody says. "Why, certainly; but you have not seen those same operations twenty-five years hence." I have patients come to me who say "Dr. Williams operated on my eye"—unfortunately he is not here. What are we to prove by that? Maybe sometime further on our patients will say the same of us. It has been my experience that we should wait until we know that all the good possible has been obtained from glasses.

Dr. Baker:

Some reflections were made about the number of cases of strabismus in Cleveland. I have seen thirty-seven cases of divergent strabismus in a practice of eight years; my good friend from Cincinnati, according to his paper, saw twelve of the same kind in the last year; so he must have seen about ninety-six in the same length of time. Therefore, I imagine that divergent strabismus is more common in Cincinnati than in Cleveland. But I wish only to emphasize this fact, that a great many of these cases will get well, if you simply put atropia in the eye. As soon as old enough they should be fitted with glasses; if you are not competent to fit the child with spectacles yourself, you ought to send it to somebody who is.

I will just say that here there is a great work for the gen-

eral practitioner to do in preventing these cases of strabismus from becoming blind in one eye; this is a point that I want to emphasize. It is the general practitioner who sees these children when they are three or four years old. I know twenty-five per cent. of your cases will get well without glasses, and that another fifty per cent. will get well with glasses without an operation.

I know the experience of all practical ophthalmologists will bear me out in this statement and there is not one here who does not know it to be true, and if he does not, he is failing to keep up with the literature of the subject, he is behind the age.

Dr. W. J. Scott:

The reason for divergent strabismus occurring in those cases which have been operated upon is, that the gentlemen who have operated on those eyes have operated on both eyes at once. It is a great mistake for anyone to operate on both eyes at once. If one eye is operated upon, the other eye ought not to be operated upon for two or three months at best. I think you will find the great majority of these cases where the strabismus has been cured, and subsequently the eye has turned outward, it has been because both eyes were operated upon at once. I remember one case where I operated a great many years ago. I thought this was a case where I would be justified in operating upon both eyes at once, because one eye turned almost behind the nose. I made the operation upon but one eye, and it was very fortunate that I did, because if I had operated upon both eyes, I would have had the other looking out of the northeast corner. No good ophthalmologist will operate upon both at once.

A RARE CASE OF PELVIC DROPSY; OPERATION; CURE.

BY J. F. BALDWIN, M. D., COLUMBUS.

Mrs. Callie Mc., of Bremen, Ohio, aged twenty-two years, was seen by me, with her physician, Dr. D. P. Adams, of this city, Dec. 9th, 1890.

I found that she had been married at seventeen, and had one child, aged three years; no miscarriages. She was well nourished, and had excellent health except in one particular: a tumor as large as a cocoanut projected from the vulva, and by contact with her clothing had become ulcerated and painful.

This tumor had been in existence for about two years. On examination we found that this tumor had for its base the posterior vaginal wall, projecting forward like an immense rectocele, for which, indeed, it had been mistaken. The tumor was further found to contain fluid, which on pressure passed up into the abdominal cavity, returning again as soon as the pressure was removed. Bimanual examination revealed the presence of two tumors within the pelvis, one on each side.

Owing to the thickness of the abdominal walls the exact character of these growths could not be determined.

The next day an anæsthetic was given, and the contents of the vaginal cyst drawn off by the aspirator. The contents consisted of simple serum, a little more than a pint in quantity.

Under the anæsthetic a more thorough examination was possible than on the day before, but not much more

could be learned. A week later the cyst contents had re-accumulated, and the tumor projected as before. The cyst was formed by a prolongation of Douglas's cul-de-sac, and our theory was that the fluid was the result of the pelvic tumors pressing on the veins of the broad ligaments.

Our patient was in splendid health, except for this ulcerated cyst, which, while not endangering life, rendered life very uncomfortable. She was very loath to have the pelvic tumors removed, as they were small and gave her no trouble, and she did not feel like taking the risk of an operation. We therefore decided to make a more thorough examination.

Accordingly, December 29th, she was again anæsthetized, and under strict aseptic precaution the cyst was incised, its contents evacuated, and the finger passed into the sac and thence on into the pelvic cavity. The cyst was then found to communicate with the abdominal cavity by an opening through which I was just able to pass my finger, situated just back of the womb. On one side was an ovarian cyst, the size of a large orange, and on the other side was a cyst of the broad ligament of about the same size.

We had now made a sure diagnosis, but what was to be done?

These cysts were adherent, though I did not think the adhesions were dense. But cysts of the broad ligament are not pleasant things to deal with even at the best. My friend, Dr. R. B. Hall, in reporting such a case a few months ago (*Lancet-Clinic*, December 20th, 1890), speaks as follows concerning these operations:

"It has been said by men of vast experience that the operation of removal of intra-ligamentous cysts is the most trying, difficult and dangerous of all abdominal operations. They are the cases which die on the table, from shock due to the prolonged operation and loss of blood; or the operator is unable to enucleate all of the cyst,

and is compelled to cut away the greater part of it, stitching the base to the lower end of the incision, and not a few thus treated die from sepsis. They are the cases that put to the test the courage and skill of the operator as no other operation with which I am acquainted is capable of doing."

I operated on such a cyst last August, assisted by Drs. McKinley, Nash, Wilson and others, and although my patient made a most excellent and prompt recovery, I fully concur with what Dr. Hall says of the difficulty of the operation.

We knew that our patient would hardly consent to so dangerous an operation as the removal of these two tumors, and yet her condition was one that demanded relief. I therefore suggested that we try to secure enough local inflammation to cause obliteration of the sac, as in a case of hydrocele of the tunica vaginalis. This course being approved of by Dr. Adams, I dipped my finger into a one to one thousand solution of bi-chloride of mercury, and repeatedly applied this to the inside of the sac. The incision was then closed by silk sutures, and the patient placed in bed. As much care was taken to secure quiet, etc., as after any other abdominal operation.

The walls of the cyst became thickened, as inflammatory reaction came on, just as we see after operation for the radical cure of hydrocele, but this was limited to the cyst, and the result was as we had anticipated: the cyst was obliterated, and the patient was cured; at least there has been no return of the trouble up to this time, and she reports herself as in the very best of health.

I report this case for several reasons:

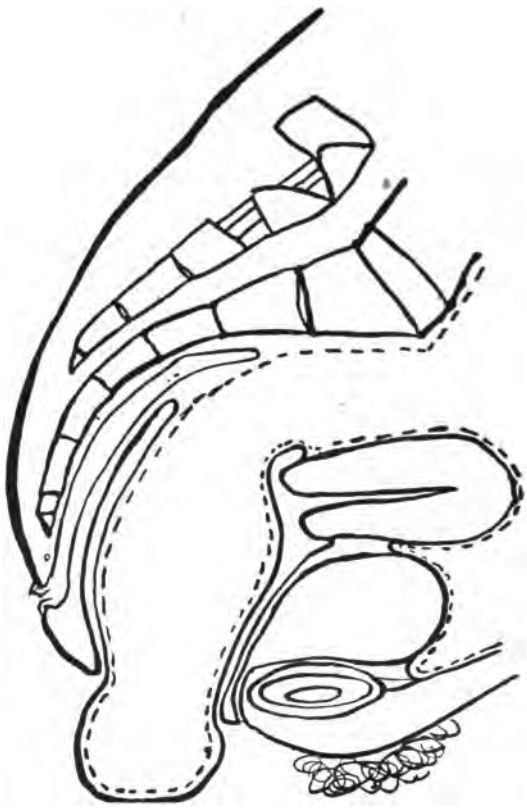
First, to ask why do we not more frequently have similar protrusion of Douglas's pouch in cases of ascites? Ascites is common enough, and this would seem to be a weak spot in the parietes, and yet I have seen but one case in which this occurred. In that case I could find no cause for this protrusion in the condition of the pelvic organs.

There were no evidences from [illegible] The [illegible] never unable to find room among [illegible]

Nelson, how remarkable that these cases which [illegible] [illegible] remarkable reading [illegible] upon which I am [illegible] [illegible] that as I remember it, there was a [illegible] of the children. What who limits [illegible] have been able to make [illegible] have no form [illegible] [illegible] course.

Time I should like to have participated [illegible] The result was successful, [illegible] *primâ facie* evidence [illegible] and I should like to hear from those [illegible] It helps and abdomina[illegible] [illegible] a fair amount [illegible] degree of success. I will [illegible] tioner, and not a specialist

The accompanying plate illustrates [illegible] tion of the tumor:



WHAT CASES SHOULD BE DRAINED AFTER ABDOMINAL SECTION?

BY RUFUS B. HALL, M. D., CINCINNATI.

Surgeon to the Cincinnati Free Hospital for Women; Clinical Lecturer on Gynæcology at the Miami Medical College, etc.

At the present time there seems to be a considerable difference in opinion and practice in reference to drainage after abdominal operations. Some operators relying upon strict antiseptic methods in their work discard drainage almost entirely. There are others who are just as painstaking in every detail pertaining to antisepsis and cleanliness who use the drainage tube very generally; and a few who drain after every section. Between these extremes there are many grades of opinion and practice. There are many reasons why drainage should be generally employed after abdominal operations. To a few of these I shall briefly direct your attention. We may have observed every antiseptic precaution in any given operation, yet who can say with absolute certainty that nothing has been overlooked, or that some break in the technique has not occurred which would cause septic changes in the fluid retained in the abdominal cavity after an operation. We may leave the abdominal cavity as clean and dry as possible at the time of closing it, yet there is always more or less accumulation of fluids during the first few hours following every operation. The fact that the retained fluid may become septic, and thus a source of danger, was observed and commented upon by Sir Spencer Wells, in 1859. The fourth case operated upon by him died in thirty-two hours afterward. At the

autopsy he asked, "Did she die of peritonitis?" There was serum in the peritoneal cavity, but he very significantly expressed a doubt if simple peritonitis was sufficient to cause such sudden collapse and death. He declares that if peritonitis killed her it was indirectly by the formation of a morbid process. In seventeen autopsies recorded in Wells' first volume, published in 1865, he reports the presence of serum within the peritoneal cavity in every case, and none of those cases were drained, and describes it as "turbid serum," "red serum," "blood-stained serum," in varying quantities, from a few ounces to one pint or more; in every case death followed in from twenty-four to seventy-two hours. In examining the report of the autopsies in Baker Brown's series of 111 cases you will find recorded that "within the pelvis was found a little bloody serum." When we recall the fact that the peritoneum is a large lymph sac, and that the fluid within is taken up into the circulation with great rapidity, we can understand why these cases are overcome so rapidly by the absorption of a concentrated septic fluid, which condition we might have in any given case where drainage was not employed. It has been the prevailing opinion until quite recently that peritonitis was the cause of death in a large per cent. of those cases dying after abdominal operations, and the leading writers of but a few years ago state that peritonitis was the cause of death after ovariotomy in from twenty-five to forty-three per cent. of all those dying after that operation. In reviewing those reports with our present knowledge of the after-management of these cases, I am convinced that those writers recorded cases as dying from peritonitis, which we would now record as deaths from sepsis, or perhaps septic peritonitis. We have devoted too much attention to the treatment of peritonitis after operations, and thus overlook the great source of danger, that of the accumulation and retention of serum in the abdominal cavity, and its rapid decomposition and subsequent

poisoning from its absorption. The late Marion Sims fully appreciated the dangers arising from this cause, and gave expression to his views in the following words: "There is no more reason why the system should not be overwhelmed by the absorption of concentrated septic fluid, than by the rapid absorption of an overdose of morphia." With these facts before us, voicing the opinions of the eminent surgeons named regarding the dangers of fluid remaining in the peritoneal cavity after operations, we are amazed when we consider the long time it has taken to establish upon a firm basis the advantages to be derived from abdominal drainage. Again, when we consider the number of operators at the present time who only use drainage in complicated cases in which there have been extensive adhesions, we are forced to believe that they have not given their patients the best chances for recovery. The amount of bloody serum removed from the peritoneal cavity after many of the simplest of the pelvic and abdominal operations, is incredible to any one not accustomed to draining after such simple operations. This fluid may be perfectly harmless, I have but little doubt, so long as no decomposition takes place, but if that occurs the patient is in great danger. It is certain that if we remove all fluids as fast as formed there will be no danger from such a source. To leave the cavity undrained for the peritoneum to dispose of the fluid, is to tax unnecessarily the powers of nature, a burden we as operators have no right to impose upon any patient, when we have such a simple and harmless procedure at our command. The fact that nature is competent to dispose of the fluid accumulation in the majority of the mild cases is no argument to use against the employment of universal drainage. Especially is this apparent when we recall the fact, which every one must admit as true, that no man can tell to a certainty in any given case whether or not nature can or can not accomplish that task. So far as I am aware

I am the first one to employ universal drainage after abdominal section. I have drained in every case of abdominal section which I have made since September 23, 1886, with but one exception; and I do not feel called upon to apologize for my results, except in one case, and that case was not drained, and the patient died. This death I am now reasonably certain could have been averted had the case been drained. The patient was aged forty, suffering from a bleeding fibroid of the uterus. The tumor was the size of a cocoanut, and dipped well down into the broad ligament. The uterine appendages were removed without difficulty, as there were no adhesions. The operation was a perfectly easy and simple one, and was made at the patient's home, and I was unable to see her afterward. As the tumor almost filled the true pelvis, I could not place a glass drainage tube so as to drain the pelvis; I decided not to drain at all. The patient died fifty hours after the operation, with a high temperature and rapid pulse, and at the autopsy several ounces of blood-stained serum were found in the peritoneal cavity. She died from sepsis. I occasionally encounter cases at the time of closing the abdomen in which it appears as though a drainage tube was not necessary; still we are treated to the most extraordinary surprises, and in not a few of these very cases, secure large quantities of blood-stained serum. Again, if there is hemorrhage the drainage tube will notify us of its occurrence, and if there should be any difficulty developed about the pedicle the drainage tube leaves a track to the surface that may be followed by any fluid which may be formed at that point. If we have placed the drainage tube in a simple case, and at the end of eight or ten hours it is found that the case is one which does not require drainage, the tube can be removed without disturbing the patient in the least, and it has done no harm. If upon the other hand this same simple case should have been one requiring the use of the drainage tube, you have put the patient in the best possible condi-

tion to recover by its use. Take the operation for the removal of the uterine appendages in long standing inflammatory diseases; the adhesions are always firm, and extensive, and no abdominal operation can test so thoroughly the efficacy and utility of drainage, as does that operation in these chronic cases. And here, where every case was drained, I have a series of thirty-four consecutive cases without a death. I mention this to illustrate the point I wish to emphasize in this paper; a point I believe which is not duly appreciated by many operators. If we place reliance upon drainage, and not upon the absorptive powers of the peritoneum, we will have no reason to regret it, and we will save not a few lives which would otherwise be lost. In this branch of pelvic surgery I am certain every operator will agree with me, that the adhesions are both firm and extensive, and on account of the large surface denuded of peritoneum these cases require use of the drainage tube for a longer time than most other abdominal operations, testing thoroughly its powers for good. In the cases referred to, I took them as they came, both the good and the bad; all were feeble from long suffering and repeated attacks of inflammation; a few were bed-ridden, and two were brought in on the stretcher. The objections to the drainage tube that have been given at various times, are that it is a source of septic infection, a frequent cause of hernia, a foreign body, a cause of irritation, and not infrequently omentum became fastened in the perforation, preventing its easy removal. As to the first objection, I have seen no case of sepsis developed from the use of the drainage tube; but it is evident that unless the utmost care is taken such an accident might occur. In my own cases I have seen two hernias developed in the line of cicatrix, but in neither of them did the hernia occur at the point where the drainage tube was placed. I have seen no appreciable disturbance from irritation of the tube since I commenced using the small, perfectly smooth tube, with no side per-

forations, which I had made in September, 1888, and used until October, 1889, when I substituted the small perforated tube of Dr. Price, which fulfills the requirement for abdominal drainage perfectly. If the dressing is so arranged as not to make pressure upon the outer end of the tube, no apprehension need be entertained as to its causing trouble from irritation, if the tube is removed just as soon as the fluid becomes straw-colored. If one employs the old style large tube, with large side perforations, there is danger from omentum becoming forced through the openings, causing difficulty in removing the tube, and possibly favor the development of hernia; but in the use of the small tube, with the narrow perforations spoken of, these dangers are reduced to a minimum. With these facts before us I am convinced of the wisdom of *universal drainage* after abdominal section. I am aware that this will be considered by many as a useless and dangerous procedure, nevertheless I believe that it is correct I have saved the lives of several patients by putting in a drainage tube where it was apparently not indicated. So long as I can say this of a single case, and so long as drainage is the harmless procedure that it now is, I shall continue to give my patients the benefit of the doubt, by draining every case in which the peritoneal cavity has been opened.

DISCUSSION.

Dr. E. I. Ricketts:

I wish to corroborate all that Dr. Hall has said as to drainage. One point I wish to speak about especially, is this: as to the length of time the drainage tube should be left in. It is a thing that can be overdone. In a number of my cases, I have not allowed the drainage tube to remain in longer than two hours.

As to the different kinds of drainage tubes: some use the Price tube, some use those of the Keith pattern. I have been unfortunate enough to have had an accident similar to that Dr. Hall describes—that of having the

omentum attached to one of the openings, and it was with difficulty that I could remove it. In one case I put a ligature around the Fallopian tube, making a figure 8 knot. You will be surprised in many of those cases in which you think you have no fluid to remove, to find from two to four or six ounces of bloody serum to be removed the next few hours. I am in sympathy with the paper—with universal drainage.

Dr. R. H. Reed:

I agree entirely in the idea of drainage. I differ somewhat from the idea suggested by Dr. Ricketts in the use of the large tube. I think the small tube is preferable, although I don't know that it is necessary to have the tube perforated. I use an ordinary rubber syringe for getting up the leakages. The old-fashioned idea of allowing the fluids to drain out, is uphill business, because it has got to go up hill to empty. Hence, I think the better method is to take a long pointed syringe, and have it go down as far in the pelvis as you can, and suck the fluids up with your syringe. If you find after twenty-four hours that you are not getting any of those secretions, and the patient is getting along nicely, I see no objection to removing the drainage. As long as you have a collection in the pelvis, I think it is proper to leave the drainage tube in. I think the paper is an excellent one, and one deserving of attention, because the matter of drainage is a very important one. Another point: I think where we do not use drainage tubes we have more difficulty—at least it has been my experience.

Dr. S. S. Thorn:

I am not very much of an abdominal surgeon, though I have done enough of that kind of work to perhaps have an opinion. I look upon drainage as a very powerful instrument for good, and so also it is a very dangerous instrument, if not treated well. When the doctor read his paper, I was rather inclined to get my bristles up on my back; but he slipped in a sentence, which was, not to allow the drainage tube to fill, but to suck up the fluid from the tube and keep it empty. I don't know that that was the exact sentence, but that was the idea that I got, through the turmoil that was in the house at the time. Most of us who listened to that paper would

be led to believe that the drainage tube is essential in all operations, and that it should be used on every occasion. If it is well used, it will do no harm in many cases; but if the drainage tube is left in the cavity without getting sufficient and industrious attention, it is a very dangerous instrument. If the doctor puts a drainage tube in there, and relies upon the tube to empty itself and keep the abdominal cavity clean, he will have accumulated there a fluid, which will be very dangerous. A drainage tube would not save, under those circumstances. But if the drainage tube is kept clean, then it will do everything, and is commendable, I apprehend, in all cases. But in general surgical work I will say that I have abandoned to a very considerable extent drainage. The use of a drainage tube is necessary or not necessary, depending somewhat on the man who uses it. The drainage tube is not necessary in wounds which may be looked upon as likely to be aseptic; but where we have septic material present, then the drainage tube is an essential. I had occasion to speak of an operation in which I removed a very considerable portion—the upper portion of the tibia. I don't know whether it was before this body or not. After I had cleaned out and thoroughly sterilized the cavity, I then closed up and covered in, so far as I could, the periosteum, and closed up the wound absolutely, making it as tight and close as I could, bringing it up by stitches, and then bathed it with iodoform, and adjusted Esmarch's bandage. The next morning I removed the dressing, because it had been soiled, and the next day I removed it for the same reason, but not so far as I did the day before. I did not then remove the dressing until the wound had healed, and the patient was able to be up and about. The drainage tube in such a case as this would be a detriment; but under the light of the sentence that we are to keep the drainage tube well cleaned out, in the proper case it is all right.

Dr. Hall:

Any man who would place a drainage tube in the abdominal cavity after any abdominal operation, and not pump it out, does not use proper precautions in abdominal operations. If we let the fluids accumulate until they run out of the tube, if the patient does not become sick and vomit, as they occasionally do—if they lie perfectly quiet, the abdominal cavity will be full and running over

before any fluid comes out of the tube: there will be one or two pints of fluid in the peritoneal cavity. But if the proper care is taken to empty the tube every half hour, if there is only half a dram of fluids, you will remove that half dram. Take the method spoken of by Dr. Reed—the one used by most operators—a long pointed rubber syringe, and that passed clear to the bottom, if there is only half a dram of fluid you can remove that half dram. The point is, to keep the tubes empty. If we wait for nature to take care of the fluid, the patient may have a hemorrhage, and bleed a pint or a pint and a half, and fill up the peritoneal cavity before you know it. But if the nurse is instructed to clean the tube every few hours, you are aware of the fact at once. If the fluid removed is pure blood—arterial blood—then the condition of the patient is at once known. It is, of course, no strange thing to have a hemorrhage after an operation; every man may have it.

OPERATIVE TREATMENT OF UTERINE CANCER.

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Not long since, I remember of reading of an argument as to whether a lawyer, believing his client to be guilty, had a moral right to defend him. The decision by a judge to whom the question had been submitted was, that he had, and that he had not only the right, but was in duty bound to advance every argument and use every artifice in his power in behalf of his client, "for," said he, "points and principles that you may think wrong may be right, and this categorical presentation to the judge may enable him to see his way more clearly." Whether this is applicable to medicine as to law, I am unable to say, but certainly from the experiences of the past it seems to have been the rule of practice. For a decade or more, a fierce war has been waging as to which of the principal operations for uterine cancer is the correct and preferable one: high amputation or total extirpation. The same tactics have been pursued as in contesting a legal case: magnifying the advantages of one, belittling the other; warping statistics, straining points, and I am sorry to say manufactured data and wholesale denunciations have played no small part in the contest. It seems to me high time that the order of things should give way to something better, that the wranglings of the lawyer should give place to the calm, dispassionate deliberations of the judge. It is only in this way that we can hope to arrive at anything like a just appreciation of the advantages and disadvantages of either and strike a balance be-

tween them. It is in this spirit that I propose to treat the subject in the few remarks that follow.

Although in the preparation of this paper I have consulted statistics largely, many of these are in such shape and emanate from such sources, that I have not thought best to introduce them. I have rather given the deductions derived from experience and research, trusting that these will more nearly approximate the truth than a logically statistical paper, which is often misleading. Of the several methods of operative procedure in cancer of the uterus, two only are worthy of consideration, namely: total extirpation and high amputation. As between these two there has been, and still exists, much diversity of opinion as to which is preferable. Undoubtedly the primary mortality of total extirpation is higher than that of high amputation and possibly, despite all efforts to the contrary, will continue to be so. It must be admitted, however, that within the last few years this disparity has been greatly lessened in favor of total extirpation, and there are many who honestly believe that the latter is a less dangerous operation than the former. Owing to the fact that most operators have arrayed themselves in favor of one or the other operation, it is quite difficult to get at the death rate in a way to make a fair comparison. As it now stands, I take it that the death rate of total extirpation is about one-half as large again as that of high amputation. I do not know of any consecutive cases of fifty or more in the hands of any one man in which the death rate of total extirpation is less than five per cent., whereas in taking the general average of cases operated on, it will amount to eight or ten per cent. As near as I can get at it the average mortality of high amputation is from five to seven per cent. In quite an extensive experience with this operation I have yet to lose my first patient. In a much more limited experience with total extirpation I lost my first patient, because it was a case totally unfit for operation; since that I have lost none. In 1887,

Martin gave as the result of 778 cases of total extirpation a mortality of 15.6 per cent. This would seem to argue that it may drop still lower, but there is a limit to all things and it may be that the limit is about reached. In writing the above I am aware that it is claimed for Kaltenbach that he has attained a record of three and two-tenths per cent. and Leopold has lowered it to two and five-tenths per cent. in total extirpation. But when we come to look closely into the matter, we find that the statistics of these men have been artfully arranged to make this showing. For instance, in the case of Leopold, we find that out of eighty cases, he had four deaths. But, they say, two of these cases were blunders and should not be counted; hence there would be but two deaths out of eighty, which would be just two and five-tenths per cent. Could we eliminate our blunders of judgment and execution, what a grand record would appear! When Leopold blunders, where shall we look for proficiency? If Leopold blunders in the prime of life, in the zenith of manly attainment, when will Leopold not blunder? Certainly the claim is not even ingenious. But the facts are he lost four cases out of eighty, making five per cent. and out of 110 cases he lost 5.55 per cent. Later corrected reports make the aggregate of his mortality six per cent., which, it is claimed, is the best general showing on record.

Price has recently given us a record of 900 abdominal sections with a mortality of three per cent., which, taken all in all, has probably never been equaled. It is doubtful if this will ever become the standard. Total extirpation, I take it, is in its very nature a more dangerous operation than simple ovariotomy. In the former we have to deal with both broad ligaments, we expose a larger raw surface, and the peritoneal cavity is brought in continuity with a mucous surface, often with foul and poisonous secretions. In the latter several of these conditions are absent or modified. While nothing will take the place of properly collected statistics for determining

questions of this kind, the conservatives advance with much confidence the *a priori* evidence pointing to the increased risk attending the more radical operation. One of the principal dangers, say they, lies in the invasion of the peritoneal cavity with all its attendant and possible evils. The danger from hemorrhage is increased because we have to deal with larger vessels in a situation where skill and prompt action are requisite to the life of the patient. Add to this the increase of shock due to the greater mutilation and jugulation of the stump, the subsequent sloughing of the same as the result of forcipressure, the danger of including the uterus in the ligation, the possibility of intestinal obstruction by adhesion, and prolapses of the intestines through the opening at the vault, and it makes a formidable array of not always avoidable complications. On the other hand it is claimed that the stump of the uterus is a much better channel for infection than can be found after extirpation, that the ligaments are sealed by ligature or forcipressure, that hemorrhage is much more apt to occur after amputation, that by strict aseptic methods the vaginal secretion need give no special trouble, and above all, that abdominal surgeons the world over regard the uterus as a kind of *noli me tangere* and hold it in special dread. The ultimate results occurring from the two methods, while favorable to extirpation, are not so markedly different as we could wish. If cancer was always an eradicable disease, we would expect much more from the radical operation than the other, but whatever we may say, or try to think, I am persuaded that very few of us believe that in the majority of cases as we find them, we can, by any operation, effectually relieve our patients of the tendency to recurrence. If then the seeds of infection are resident after our most radical operations it stands to reason that there can be no great difference as to time of recurrence between cases in which the main center of infection has been eliminated and those in which a wide area of tissue is included, so long

as the infiltrating element lies beyond the reach of the knife. I do not wish to be understood as maintaining that cancer is an all-pervading, ineradicable disease, for the history of the past affords abundant evidence to the contrary, but until such time as we can perfect ourselves in diagnosis as to be able to recognize the first approaches and the habitat of the disease, and until a regular and methodical espionage shall have been established over all women between the ages of forty and sixty, the majority of cases will slip through our hands, despite of any and all operations. Total extirpation is especially applicable to cancer in its earliest stages and in those in which it confines itself to the mucosa. This it does in a certain proportion of cases, but in another large proportion it extends by preference to the broad ligaments; once there, I believe the case to be hopeless; it will recur sooner or later. Hence the operation is applicable only to a limited number of cases as we find them, and we are perforce compelled to resort to other measures. When the infiltration is extreme, when it obviously involves the broad ligaments, when the uterus is immovably fixed or when tumors are attached to it, where the vagina is narrow and deep or where there is marked contraction of the pelvis, this operation is relatively or absolutely ineligible. Under many of these conditions high amputation is still available. But other conditions demand consideration and these are of no small consequence. The increased mortality due to the operation, *per se*, will have its influence in restraining patients from submitting to operative interference and will consequently stand in the way of securing for them the maximum amount of relief. Doubtless it will also stay the hand of the timid surgeon, for it requires no small degree of courage for a man to insist upon, and execute an operation of this kind, on a patient who, as yet, has given no evidence of physical deterioration. Especially is this embarrassing in private practice, where the patient, a pretty and vivacious wo-

man, in the midst of a large circle of influential friends is suddenly pounced upon by the surgeon with at least a prospect that the next change in the episode will be a funeral. But this should not weigh against duty. With the same unwavering trust the explorer exclaims, "There is the west—there is the Pacific," should the conscientious surgeon follow the beacon-light of truth and duty. Again it is pretty generally recognized that with recurrence the sufferings of the patient are less, the farther the disease is removed from proximity to the bladder and rectum, and the good results of the operation more conspicuously manifest. Notwithstanding the very evident advantages of the more radical operation, I cannot agree with that pessimistic view which clamors for extirpation or nothing.

It is claimed that as the cases run, amputation only secures for the patient a short respite, and the surgery that deals in such methods is opprobrious, that it works discredit to our calling. But statistics do not bear us out in this view. Let us look into the matter. The chances are that a woman will live about as long after the recurrence of the disease succeeding an operation, as she would have lived had the disease been allowed to pursue its course unmolested. Then all the time between the date of the operation and the date of reappearance is clear gain. But this is not all; from the time of operation until the disease recurs, the patient is exempt from all suffering, a new lease of life has been conferred, hope and cheerfulness take the place of anguish and despair, while happiness pervades the household over which the pall of an impending doom so recently hovered. Then when the disease returns, it has been robbed of many of its terrors, because its habitat has been removed above the zone of greatest sensitiveness, the discharges are less abundant, the odor materially diminished. Again, if we are to have high amputation or nothing, how are we to deal with that large class of patients in which extirpation is impos-

sible and high amputation still feasible? This dog in the manger policy is not scientific, it is not humane. The only case I ever lost after operation for uterine cancer, was one in which I extirpated where a large subperitoneal fibroid was attached to the fundus of the uterus, and which gave rise to a tremendously difficult and prolonged operation. Had I been content with amputation, I am sure my patient would have survived many months. The disposition to garble statistics has been quite general on both sides. In settling cases of this kind, it will not do to base our deductions on phenomenal records, but a general average must be sought, and deductions made from these. To illustrate: Byrne claims that he has no mortality due to the operation *per se*, and I believe that Baker makes a similar claim. Others, doubtless, have enjoyed a like experience. As to ultimate results, Byrne gives a phenomenal record of longevity, and Baker claims that sixty per cent. of his cases are cured. This for amputation. On the other hand, out of a number of cases, some operators can show a primary mortality ranging from nothing up to two or three per cent. for extirpation, and out of eighty cases recently tabulated taken from different operators, about seventy-eight per cent. of the patients were alive at the end of five and one-half years. With his eye fixed on such statistics, Schauti avers that forty to fifty per cent. of cases are cured by total extirpation. Whereas, another statistician upsets all this by averring that in Berlin every case of total extirpation was dead at the end of four years. That extirpation, in some cases at least, much more effectually fortifies the patient against relapse than amputation I am just at this minute forcibly reminded by a letter and a gift from a patient, for whom one year ago, I amputated the cervix for cancer, but which returned in six weeks. I then removed the uterus entire, thinking only to give my patient a respite of a few months. She now writes that she is well and attending to her household duties.

From the foregoing I think it will appear: 1st. That the primary mortality of high amputation is somewhat less than that of total extirpation, but that even in this respect, they are moving toward each other year by year. 2d. That the ultimate results occurring from total extirpation are better than those by high amputation. 3d. That in quite large classes of cases in which the disease follows the direction of the uterine canal, total extirpation is the operation *par excellence*. 4th. That with the present means and lack of diagnostic facilities, we are often unable to differentiate this class of cases from others. Therefore it would be safer in all amenable cases to remove the entire uterus. But many cases are not amenable to extirpation, and must of necessity be relegated to that category for which amputation offers the only recourse. Or in short: extirpate where you can; amputate when you must.

DISCUSSION.

Dr. W. H. Humiston:

I was very much interested in the paper indeed. I think he has compiled a very valuable statistical paper. But the question occurred to me in these cases of carcinoma cervicis uteri, where you cannot induce the patient to submit to high amputation for the removal, to laparotomy, would it not be good practice to remove it by cautery? I had two cases in which I could not induce the patient to submit to an operation. I thoroughly cauterized. I examined that case this past week. She has no more hemorrhages, she has no more pain, and she is looking quite well, and losing cachexia.

The second case was a more serious one. In that case also I was unable to operate. I treated it in the same way. What the result will be I cannot say. I had hoped that perhaps Dr. Reamy, with his large experience in these cases, would give us some treatment that we could adopt when we could not induce the patients to accept high amputation.

Dr. F. D. Bain:

I want to ask the doctor if he would use cautery in a

case where the carcinoma has gone farther than the uterus?

Dr. Humiston:

I certainly should not. I certainly should not advise any operation whatever under those circumstances.

Dr. Reamy:

I dislike to take so much time, but as I probably will not take any of your time to-morrow in discussing anything, and as I perhaps have had more experience in this particular subject than I have in some others, I will call attention to one or two points in this admirable paper. I was very much pleased with his thoroughness, and with his conservatism; but I scarcely think that the facts justify the inference that one would draw from the reference to the work of Byrne, of Brooklyn. I may misunderstand Prof. Gilliam on that point, but I interpret what he said as expressing his doubts as to the accuracy of these statements of Byrne as to the remarkable results he has obtained by the use of cautery in these cases. Am I misunderstanding?

Dr. Gilliam:

I think from these cases we ought not to adduce any conclusion.

Dr. Reamy:

I think I was justified in this interpretation, because he announces in the beginning of his paper that there are but two methods worthy of consideration: one is the total removal of the uterus, and the other is high amputation; therefore I think I am warranted in the inference that in his reference to Byrne's work, that Byrne was mistaken in his conclusions. Anyone who has seen Byrne work, and knows, first, his character and capacity to know what he is talking about and how to do it, and then his striking honesty, will not hesitate to say that Byrne is certainly sincere. And I believe that the present state of this question justifies the deduction that in cautery there is a remedial agent in many of these cases which heretofore has not been used to the degree that its value would justify. I have now in my charge a woman who has an ordinary cancer of the neck of the uterus.

When I saw her it was too far along for high amputation of the cervix, and I thought it was too far to justify total extirpation. I have had an experience with twenty cases only of total extirpation; I have had an experience with sixty cases of high amputation, fifty of which I have reported, with a success that amounted to twenty-two cases. My statistics are published in a paper which I read before the American Gynecological Society four or five years ago, and I will furnish you ten or fourteen of these women still living, some of them ten and fourteen years after high amputation of the cervix. This case that I refer to now is a patient living in Lexington. I used the cautery in that case four or five times, carrying it up into the uterus, sweeping it around. Mark you, the cachexia is not nearly all gone, but the uterus is very much more movable, and I believe that there will be such a mobility of the uterus, that if I want to do it, I can remove the entire uterus, as a result of the cauterization. In a large number of cases the only thing to do is the total extirpation of the uterus; but the difficulty exists in the fact that it is impossible for any man to say that the disease has not extended out between the folds of the broad ligament, that it has involved the broad ligament. Too many of my cases show, where I did the work well, that the disease extended beyond where I could go, or any man could go, to cause me to believe otherwise. So while I do not doubt the truth of the gentleman's report of such marvelous results—it is a marvel to me,—I cannot understand why men make statistics of one, two, three and four per cent. of death, and in those cases the disease hasn't extended into the fold of the broad ligament, and don't return with that, and why it comes back in my cases: that is what bothers me. My impression of it is, that the difference in the statistics arises from the fact that a large number of these women who furnish the subjects for total extirpation were women in whom the disease was only, at the time the operation was done, still in the intra-vaginal portion of the cervix; and I repeat it, that in such a case amputation of the cervix is as efficient as the removal of the whole uterus, because it is confined where it commences, to the fundamental epithelium of the cervix, and has not entered the uterine cavity—there is no connection between the cervix itself and the body of the uterus. Those cases will recover. You may remove the uterus,

if you prefer the operation. As my friend has suggested, it is a little more dangerous. I don't believe it is any more dangerous. I remember once I said that to the other side, if you will allow me to say that I am on one side, but I am not, for I believe one operation about as safe as the other. But my objection is, first, that the statistics are not reliable, because they are predicated upon the removal of the uterus in cases that could be treated more easily by the high operation; and secondly, that these cases are not watched long enough where the disease has extended outside, in order to allow them to testify in this court at all. If a woman must die of the disease by recurrence, in six, or eight, or nine months, or a year, that woman would live longer if it was conservatively cauterized, and allowed to be let alone. Therefore these cases ought to be in condemnation of total extirpation. It is the value of the conservative method in some of these cases where there is an expectation that the disease has gone beyond this point, in retarding the progress of the disease, and prolonging life, which is a good deal. These gentlemen seem to forget, many of them, that these women will live from two to three years, if they are not touched, from the time the disease first shows itself.

Dr. Gilliam:

I have but very few words to say in conclusion. I think I have fortified myself very well in the paper, being conservative in this matter. When I started to write that paper, I started with a view of rather defending high amputation in all cases in which high amputation could be done; but as I progressed, and looked over the field more thoroughly, I became convinced in my own mind that in that class of cases in which the infiltration follows up and under and along the uterus, following the trend of the uterine canal, and we are unable to diagnosticate just at what time we should perform high amputation, and that form of high amputation in which you take out the uterus clear to the fundus, we should perform extirpation, which we can do with almost as little danger to the life of the patient, if not as little danger, as high amputation, and give her a chance. Perhaps the first of my paper was unfortunate in saying that these two operations of high amputation and extirpation are the only ones worthy of consideration. I will take that back, for I do believe that in some cases cauterization is very efficient.

TWO CASES OF EXTRA-UTERINE PREGNANCY.

BY WILLIAM D. HAMILTON, M. D., AND CHARLES S. HAMILTON, M. D., COLUMBUS.

Case I. Extra-uterine pregnancy; abdominal section in the twelfth month; delivery of a dead fœtus; recovery.

Mrs. F., seen with Dr. I. N. Beach, of West Jefferson, Madison County, Ohio.

She was forty-four years of age, had been married twenty-five years, had had two miscarriages and six children, the last in August, 1884. Her health was perfect in November, 1889. Amenorrhœa existed from January to August inclusive in 1890. She had morning sickness during the early weeks. For a fortnight or more in July, she had a free discharge of water from the vagina. Her breasts filled with milk in the later months and dried in September. She frequently had pain in the left groin, sometimes aching, at other times darting across the abdomen, and very severe. Especially was this true during one week in August. Quickening, which had been well marked, suddenly ceased after the pain subsided. Then followed free, bloody discharge for forty days. Slight hectic was present in September.

The patient was slender, swarthy and cachectic. The tumor, which had appeared within the previous few months, was non-symmetrical, the left lower segment of the belly being more prominent. It was generally fluctuant or semi-solid. The cervix was slightly enlarged and fixed high behind the symphysis. The os was moderately open. The expanded cul-de-sac apparently contained fluid.

Diagnosis: Multilocular cyst? or extra-uterine pregnancy.

Operation, November 11, 1890, in the presence of one hundred students and physicians. An incision four and a half inches long exposed a tumor, which when perforated yielded a small amount of serum. A finger introduced through the opening in the sac felt a leg, upon which traction was used in delivering a macerated fœtus, weighing nine and a half pounds. The child had hydrocephalus and spina bifida. The placenta, slightly to the left of the incision, was not disturbed. The cord was tied and cut, and the stump of it was left protruding. Hemorrhage was slight. The sac varied in thickness from one-half to three-fourths inch. Very little fluid escaped into the peritoneal cavity. Silk-worm gut was used for parietal closure at the upper end, and silk stitches for uniting the sac to the lower two-thirds of the external incision. Free washing of both general and special cavities was employed. Drainage tubes were inserted at the lower angle and near the cord. Little or no shock followed. The after-treatment consisted of rectal feeding with peptonized beef extract, white of egg and wine for three days. The sac was washed out every four hours, alternately with boric and iodine water. The bowels acted in reponse to salts, on the third day. Placental debris came away with every washing. No abdominal distension was present, except that due to the presence of the placenta. December 1st the wound was discharging slightly and was smaller. December 4th (23d day after operation) the patient was presented before the Central Ohio Medical Society. December 7th she left the hospital well.

COMMENTS:

Why this obscurity in diagnosis?

1. A large macerated hydrocephalus may feel through the cul-de-sac like any other collection of fluid, so that the head may be difficult to recognize as such.

2. The woman thought she was pregnant. A patient

who has borne children can give more reliable information about changes in her breasts and the kick of a fœtus than one who has had no such experience.

3. Spurious labor, with the death of the fœtus, explains the final cessation of the pain in August.

4. This case probably belongs to the intra-ligamentous type of tubal pregnancy.

Case II. Ruptured tubal pregnancy. Patient in collapse twenty-four hours afterward; abdominal section by Dr. Charles S. Hamilton; recovery.

Mrs. J. J., of Columbus, Ohio, was admitted to Mt. Carmel Hospital, 3 P. M. February 7, 1891. She was first seen by the writer at her home at 2 P. M. on the same day. She was thirty years old, had been married ten years and had conceived four times prior to this illness. The last and only birth at term was in 1885. She had had leucorrhœa for seven years, with considerable abdominal tenderness at times. The breasts were somewhat enlarged and tender in the last few weeks. She had had some nausea and frequent vomiting. December 20, 1890, shreds were discharged from the vagina, and on the next day there was a gush of blood, with pain in the back. On February 5th (her menstruation being absent) there was an intermittent, pink, watery vaginal discharge for twenty-four hours. She was pale, anxious, pinched and had cold extremities. The pulse was 136 and thready. Her lips were blanched and her face and ears were cold. She complained of severe pain in the left hip and left breast, which began at 8 o'clock on the preceding evening, February 6th. It was sharp, continuous, severe, so that, as she stated, she "could not straighten out." The lower abdomen was prominent, tender and dull on percussion. The os was slightly patulous, the cervix was fixed, and normal in position. The fundus was not discernible and "Douglas's pouch" was tender.

Diagnosis: Ruptured tubal pregnancy. Abdominal section was done by my colleague, Dr. Charles S. Hamil-

ton, in the presence of Dr. N. R. Coleman and Dr. Edwin F. Wilson. Although the patient was filthy, we thought it unsafe to do more by way of preparation than to wash the abdominal walls and vagina before etherization. Her pupils were dilated, the pulse was so feeble as to be hardly appreciable. Her deadly pallor and her bloodless abdominal walls reminded us of an autopsy. The amount of tarry blood and clots washed out was estimated at one gallon. The diagnosis was promptly confirmed, and the ruptured tube, large as a hen's egg, was tied and cut. The pulse improved under the hot water washing. A drain and stitches finished the procedure. She had no abdominal symptoms of importance. She was discharged March 13th. Her convalescence was retarded by a burn on the foot from the application of a hot bottle during anæsthesia. The writer has published four cases of extra-uterine pregnancy in the *New York Medical Journal*. In the three cases in which abdominal section was permitted, the patients recovered. The five women are all alive and well to-day.

ON THE VALUE OF DRAINING THE PELVIS WITH LARGE TUBES AND IODOFORM GAUZE STRIPS IN CASE OF BLEEDING AFTER LAPAROTOMY.

BY M. STAMM, M. D., FREMONT.

For the last six years I have used the iodoform tampon in various cases of hemorrhage where I could not reach the blood vessels well enough to apply a ligature, and I can only say that it has relieved me of considerable annoyance and anxiety. Mikulicz, about five years ago, introduced his tobacco-pouch tampon in cases of pelvic operations to cover, as he called it, the dead spaces resulting from enucleation of large tumors which had no pedicle and where the peritoneum was not extensive enough to cover the enucleated surface. This was certainly a very valuable addition to our operative measures and it encouraged quite a number of surgeons to remove tumors which otherwise they would hardly have dared to touch. The only objection I found, in one case of laparotomy, was the difficulty of its removal. When, therefore, Walcher, of Stuttgart (Centralblatt fur Gynækologie, No. 46, 1890), described his method of draining the abdominal cavity in cases of hemorrhage after laparotomy, which he had tried in seven cases with excellent results, I was not slow in my resolution to carry it out should a case come into my hands which would give the slightest cause of apprehension in regard to after-bleeding.

On February 1st, this year, I operated on a case of double ovarian cancer in an advanced state. The tumors

were so wedged into the pelvic cavity that their pressure upon the blood vessels produced such enormous ascites as to threaten her with suffocation. The ascitic fluid again pressed the tumors into the pelvis that the patient was frantic with pain in the small of her back. To give her relief I tapped her once, but in course of one week the fluid had reached the former volume and the excruciating pain in her back, as well as the signs of suffocation, returned with the same severity. The patient, who at first told me never to speak of an operation, now imperatively demanded to be relieved of her suffering in whatever way and at whatever cost. I only reluctantly yielded to her desire, the more so as I had noticed a nodule in the epigastric region at the time when I tapped her. On incision the right tumor was found to be movable and presented a pedicle, but the left one was adherent to the sigmoid flexure and pelvic wall so that I had to peel it off. There was considerable parenchymatous oozing, which did not cease after flushing the cavity and using pressure with sponges, so that I decided to use a large rubber tube about the thickness of a thumb and a strip of iodoform gauze about one and one-half yards long. The latter was pulled through the tube and the long end pressed against the bleeding parts, the Douglas pouch was filled by it and the tube kept the pressure up. She recovered from the operation without any untoward symptom, but about four weeks later showed signs of pyloric obstruction, and at the end of six weeks she died from inanition.

Removal of a Dermoid Cyst.—Dr. Wm. Busch, of Genoa, O., called me to see Mrs. T., aged about thirty-three years, married for several years, never had any children. She complained of pain in her right ovarian region for the last few years, but especially so since last July. On examination I found a solid, not fully outlined tumor, reaching nearly up to the umbilicus; it moved very little and with the uterus only. At the operation, performed on March 25th, the tumor presented the size of a large

child's head; adhesions were extensive. An incision into the tumor brought to light a large quantity of soft, cheesy substance, with balls of long hair. As it was impossible to form a suitable pedicle, I peeled the tumor out of its peritoneal covering and then tied the remaining mass in four parts, but there was still some bleeding going on even after flushing the cavity and using pressure with sponges. The light of the room was very unsatisfactory, and even after giving patient Trendelenburg's position I could not find the bleeding point. Patient collapsed considerably so that any further attempt to find the source of hemorrhage would have been fraught with danger. I therefore filled the Douglas pouch with the iodoform gauze in the manner described in the first case. Patient rallied from the operation with a very feeble pulse, but her temperature did not exceed 100°. After forty-eight hours I removed the tube and gauze strip, and from that time on she made a rapid recovery.

I am well aware that some of our foremost German operators are not in favor of drainage, even in the most complicated cases, and that in spite of this they have obtained excellent results. A few, however, have returned to drainage, and Saenger especially advocates glass tubes with iodoform gauze (capillary drainage); he claims a smoother recovery for his cases and thinks that by this procedure the action of the blood ferments is neutralized. The majority of American and English surgeons are greatly in favor of drainage, and the good results obtained thereby will probably not lead them so soon to abandon this method. It is true, the splendid results obtained by Ohlshausen, Martin and others, without drainage, are apt to lend additional lustre to their operative skill. But as we cannot all reach such a height of perfection in operating or possess the marvelous dexterity of a Tait, and as we are not all surrounded by such a staff of well-trained assistants and nurses and other facilities which their hospitals afford, we often look about us for measures which

will shorten and simplify our operations without lessening their safety. It is self-evident that the old surgical rule, to tie every bleeding vessel which will admit of ligation, should never be thrown overboard. Despite of this rule, however, we often have cases which are in such a state of collapse that the shortening of the operation becomes imperative, and it is in such cases, especially in parenchymatous bleeding from the denuded surfaces, where the packing of the pelvis serves us in good stead. Yes, it is really for the purpose of tamponade more than for drainage that we use this combination of gauze strips and large drainage tubes, since a healthy peritoneum generally serves already as an excellent draining surface. Still in many cases the power of absorption of the peritoneum is very much impaired, and it becomes desirable to rid the abdominal cavity as soon as possible from the blood and accumulated secretions. In such cases, where drainage is used the recovery is avowedly less interrupted or eventful than where drainage is omitted. Walcher has used this method in four cases of tumors with vascular peritoneal adhesions and in three cases of ectopic gestation with the best results; he did not even make an attempt to tie the bleeding vessels. He uses glass tubes with a lumen three-fourths to one inch and of a length from five to eight inches. As I had no glass tube of such size on hand I used a rubber tube, but I think that glass tubes are preferable; the drainage opening seems to unite better after using the latter. The iodoform gauze strip can be used from one to three yards long; to exercise sufficient pressure upon the bleeding surface, you can pack the whole pelvis with it, and the tube will keep up the pressure and give ready outlet to the secretions. I applied the sutures the same way as if there had been no drainage tube inserted, and tied the thread near the tube in a noose so that after forty-eight hours, when the tube was removed, I could simply draw the edges of the opening together and make a permanent knot. In removing

the tube you lift it slightly, and you can then readily pull the gauze strip out through it. Walcher found the wound healed completely in nine days. In my cases it took a few days longer to heal, which I attributed to the use of rubber instead of glass tubes. I covered the wound and tube with iodoform gauze and wood wool. Walcher had to resort in two cases after forty-eight hours again to the tamponade, as he met with some fresh hemorrhage, but could close the wound after the third or fourth day. He also found, that owing to the pressure of the tampon, two patients complained of retention of flatus; this is, however, a small disavantage in comparison to the great advantage of the method in other respects.

SALPINGITIS; WITH A REPORT OF TWO CASES.

BY A. B. WALKER, M. D., CANTON.

It has been but a few years since we have known much regarding diseases of the Fallopian tubes. Thirty-three years ago Bernutz in his writings clearly described the pathology of pelvic inflammations. But little advance, however, was made until Mr. Lawson Tait, in 1872, made known his remarkable success in removing diseased tubes by laparotomy.

The writings of Battey, in America, and Hegar, in Ger many, on the removal of ovaries for other diseases than ovarian tumors, had much influence in developing the proper method in treating diseases of the uterine appendages.

It was not until 1882–83, in America, that the operation of laparotomy was made for removal of diseased tubes, the first operator being W. Gill Wylie of New York.

During the years 1883–84, numerous cases were operated upon for salpingitis by gynecologists and surgeons, since which time the operation has been very frequently performed.

Previous to the investigations of such men as Bernutz, Tait, W. Gill Wylie and others, characteristic cases of salpingitis were very common and went under the name of pelvic cellulitis. W. Gill Wylie claims it was true in this country until before 1882–83, since which time we find pelvic cellulitis to be a rare disease, and very seldom existing except as a phlegmon in puerperal cases.

Many of the cases we formerly looked upon as fresh

attacks of old pelvic cellulitis we now find to be local peritonitis due to salpingitis.

Salpingitis does not have its origin in the Fallopian tubes, except in rare cases, but is usually the result of extension of disease from the endometrium.

W. Gill Wylie says: "It is difficult to conceive a salpingitis starting up as a primary disease independent of any disease of the uterus or ovaries. It is generally conceded that it is due in almost all cases to the extension of inflammations from the uterus to the Fallopian tubes, and that endometritis, whatever be the cause of it, is liable to result in salpingitis."

According to Wylie's ideas, which are doubtless correct, anything that will cause endometritis may by extension of the inflammation produce salpingitis, as gonorrhœa, sepsis after abortion and labor, syphilis, tuberculosis, pessaries (stem in particular), unclean sounds, intra-uterine injections, sponge and other kind of tents.

Salpingitis may also be caused from tubal pregnancy. Anything that obstructs drainage from the uterus or tube may cause distension of tubes and result in salpingitis. Salpingitis, it is claimed, accompanies some of the eruptive fevers. Direct extension of inflammation from diseases of ovaries, as a suppurative dermoid cyst, may cause salpingitis. Pelvic hematocele and cancer may also produce salpingitis. Suppressed menstruation from wet feet and cold, may set up catarrhal endometritis, and by the narrowing of the cervical canal, which is generally the result of uterine catarrh, the proper drainage from the uterus is prevented, when the catarrhal secretion may be forced into the Fallopian tube or tubes and cause salpingitis. As the mucous membrane lining the tube takes on the catarrhal form of inflammation and the secretion becomes excessive, more than is drained off through the uterine end of tube, some of it escapes at its fimbriated extremity into the peritoneum, by which the

tube or tubes become closed with adhesive inflammation, when a typical salpinx will follow.

Chilling of the body from eating ices or drinking cold or iced water during menstruation may in like manner set up salpingitis.

As we have learned that salpingitis is such a common condition, it should always be looked for in pelvic inflammations, for it is in a large majority of cases the cause of the trouble. A diagnosis, however, is very difficult when in the acute stage. But as the salpinx becomes well developed it can generally be easily diagnosed after the acute symptoms subside.

The acute attacks women are subjected to, who have salpingitis, are usually local or pelvic peritonitis, caused by septic matter that escapes from the fimbriated extremity of the diseased tubes or from a rupture of the tube. Any woman then that is so unfortunate as to have a salpingitis is in constant danger of an attack of peritonitis from the escape of septic fluid into her peritoneum, as I have described.

Catarrhal salpingitis when the tube is not distended cannot be made out by the sense of touch. In fact it is only after adhesions or occlusions of the tubes and they are distended that a diagnosis can be made.

The symptoms of salpingitis are frequent attacks of local peritonitis; severe pain one week or ten days before menstruation; relieved after the flow is established. There is a dull, dragging, burning pain over seat of the tube affected. To the sense of touch a well-defined tumor can usually be detected by bimanual examination. If it is a hydro-salpinx, the tumor will be conical shape, walls of tube thin and elastic The local pains and acute symptoms not so severe as in pyo-salpinx; when if it is a pyo-salpinx the walls will be thick and inelastic; tumor club shaped.

Women who have both tubes affected are always sterile; when but one tube is affected and both ends of diseased

tube closed, pregnancy may take place, but an abortion is very likely to follow.

To illustrate the two varieties just described, will report the following cases:

Case I.—Mrs. Y., aged thirty-four years, married, good family history, enjoyed fair health, and menstruated regularly without pain until nineteen years old, when during her menstrual period, she drank freely of iced water, when she was very warm while working in the harvest field. From this she took a severe chill and had suppression of her menses, attended with pain, chills and fever. A catarrhal discharge from her uterus very soon set in; she suffered intensely from dysmenorrhœa at each period, not being entirely free from pain at any time. She was married a few months after drinking the iced water but lived a sterile life. Her suffering continued, sometimes worse, then again better. The pain was mostly confined to her left side and was of a burning, throbbing nature. She would have frequent attacks of what she called "inflammation," which were doubtless local attacks of peritonitis from salpingitis. Suffered at times from cephalalgia, which was doubtless reflex from her uterine organs. Many different physicians prescribed for and treated her with no permanent benefit. The only relief she experienced from pain was while under the influence of morphia. The 20th of August, 1890, fifteen years from first attack, I was called to visit her at Sparta, Ohio, in consultation with the attending physician, Dr. J. F. Fox. We found her confined to bed, very nervous and weak. Upon examination her uterus was found to be stationary in pelvis from adhesions, slightly retroflected, with marked stenosis of cervix. A well defined tumor was located in her left side which strongly resembled a salpinx. An operation for the removal of the diseased tube or tubes was recommended as the only means of relief, which was readily consented to. She was then put upon a course of tonic treatment and re-

quested to exercise daily in the open air. Two months later, October 30th, after a thorough preparation of patient, I opened her abdomen, assisted by Drs. J. F. Fox, E. O. Morrow, W. H. Becher, T. H. Brannan, C. H. Goodrich and D. L. Gans, and found a large hydro-salpinx on left side, with a small hematoma of left ovary. The salpinx was adherent in the lateral side of posterior culde-sac and to the broad ligament, and was with considerable difficulty removed on account of the adhesions. The ovary was also removed. The pedicle was tied with a silk ligature and dropped. The right ovary and tube seemed natural and were left. There being no hemorrhage of any account from the torn adhesions, the abdomen was closed without a drainage tube, silk sutures being used. She made a good recovery, now weighs 140 pounds, a gain of twenty-one pounds since the operation, menstruates regularly and without pain.

Case II.—Miss B., aged twenty years, unmarried, family history good. At the age of 16 years she aborted at five months, from which she claims to have fully recovered. Two years later she contracted gonorrhœa, from which she did not recover, but was left a physical and mental wreck from pyo-salpingitis, the result doubtless of gonorrhœal infection. December 8, 1890, I was first called to visit her, when I found upon examination a double salpinx, both of them closely adherent to the uterus and adjacent parts. They felt firm and inelastic and so closely united to the uterus that they seemed to be a part of it. She suffered continued pain, worse during the menstrual period. There was a purulent looking discharge coming from her uterus.

December 12, 1890, I opened her abdomen assisted by Drs. T. M. Johnson and J. S. Pyle, when we found my diagnosis to be correct, a double salpinx, the left one adherent to the lower part of the pelvic floor, as well as to the uterus, and it was with great difficulty that the adhesions were broken up. The right one was highe·

up in the pelvis and adherent to the uterus and broad ligament. The adhesions were also with difficulty separated. They were both ligated with iron-dyed silk sutures, close to the uterus, and pedicles seared with a galvano-cautery and dropped. They were so matted together by adhesive inflammation that we could not distinguish the ovary from the tube. In fact they cannot now be separated, as you can see by the specimens. As there was but little hemorrhage, no pus escaped into the peritoneum; the abdomen was closed without a drainage tube. Iron-dyed silk sutures were used.

Patient made a good recovery, and is to-day well and hearty, weighs heavier than ever before and is perfectly free from pain; menses regular, and unattended with pain, but they continue from seven to ten days. There is still some little discharge from her uterus.

Case I.—The cause of the salpingitis was evidently catarrhal endometritis, which was the result of suppression of her menses from drinking the iced water. Had this catarrhal endometritis been treated early by the proper remedial means, salpingitis might have been prevented and the poor woman saved many years of suffering and the loss of an ovary and tube.

Case II.—The cause was a specific one, quite different from Case I, and should teach us a lesson, that gonorrhœa in the female is a more serious disease than we formerly looked upon it to be, for the disease may extend from the vagina to the uterus and from the uterus to the Fallopian tubes and the peritoneum.

DISCUSSION.

Dr. W. H. Humiston:

Why do you think drinking ice-water causes salpingitis when the first case was unilateral?

Dr. Walker:

For the same reason that we have more trouble on the

left side of a woman. Women more frequently have ovarian trouble on the left side.

Dr. Humiston:

I would like to ask how you determine that ice-water causes salpingitis, in those cases where a woman drinks ice-water during menstruation. It is a new pathology to me; I would like to have that point cleared up.

Dr. Walker:

In my paper I gave a clear typical case of a healthy woman, working in a harvest field, very hot, taking a glass of ice-water, from which she suffered a suppression of her menses, and from suppression of menses, endometritis, and by an extension of the inflammation from the endometrium to the Fallopian tube, she had salpingitis. I am very glad that the doctor brought that out. I would like to hear from Dr. Reamy on that subject, and whether he thinks salpingitis is likely to result from endometritis from the drinking of ice-water, the ice-water producing endometritis. I think this is a typically clear case, where the endometritis was brought on by drinking ice-water; and of course the salpingitis was simply the result of the endometritis.

Dr. Reamy:

I would be very glad to discuss this paper, but I am old enough to know that a man can talk himself out. This society don't want to hear one man all the time. I would like to discuss this subject to-morrow or some other time. I don't think it is good taste for me to stand here and talk all the afternoon. I will simply answer that particular inquiry by a statement well known to everybody, I suppose, that anything which profoundly affects the nervous system, as drinking very cold water—ice-water—by a patient who is in that very excited nervous state that every woman is in when she menstruates, might arrest menstruation temporarily, or certainly very largely interfere with it, and thus suddenly arrested, it might develop endometritis, and this develop an inflammation of the lining membrane of the Fallopian tube. I can conceive of it, and I have no doubt it is so. While I believe every word of the paper, while I think without doubt salpingitis frequently follows endometritis, I do not believe that the disease in a serious form is nearly so prev-

alent as is now being taught. I do not believe, as I heard a distinguished gentleman from Philadelphia not long ago in a medical society say, that thousands of women in this country are dying from inflammation of the tubes, because surgeons are too timid to relieve them. I do not believe that. I believe it is true that some women do die. I believe both of those cases justified the operation. It was sound, scientific, well-executed surgery. But the question arises—you heard the statistical references to when the first operation was done by Dr. Gill Wylie in this country. In the name of God, how did the country become populated prior to that time? Was gonorrhœa any less frequent then, than now? Was ice-water less frequently used than now? Did women have deranged menstruation less frequently then, than now? Did women in those days have Fallopian tubes? If every other woman had salpingitis, every fourth of all these pyo-salpingitis, and every woman was sterile and would not get well without removing them, how is it that you and I were born, or had any mothers? Now, the truth of the matter is, this whole business is enormously exaggerated. I have seen tubes—not like these specimens—and I have seen ovaries presented when they were exactly in the condition of the ovaries of almost every woman at the first bearing of children. The fact of the business is, you cannot find a pair of normal ovaries in the community, and there are gentlemen who are determined to spay every woman in the land who is not labeled, before she goes on the street, 'My ovaries have been removed."

REPORT OF TWO CASES OF ABDOMINAL TUMOR, ILLUSTRATING THE UNCERTAINTIES OF DIAGNOSIS. — SPECIMENS EXHIBITED.

BY THAD. A. REAMY, M. D., LL. D., CINCINNATI.

I desire very briefly to call the attention of the Society to the specimens before you and make some remarks as to the cases to which they belong. This specimen I obtained from a woman 69 years of age. The operation was made May 28th, 1891.

I had seen the woman in consultation occasionally during the past three years. For four years prior to the operation she had been as large as a pregnant woman at the eighth month. It is seven years since the enlargement first commenced. When I saw this woman three years ago the tumor was nodular, very irregular and very firm. No fluctuation could be detected at any point. She was then as large as a woman at term. The uterus was lifted up out of the pelvis. The sound measurement showed the uterine cavity to be five inches. The tumor was movable from side to side. Movements of the uterus were likewise communicated to the sound in position. She had frequently had slight attacks of uterine hemorrhage.

My diagnosis was interstitial uterine fibroid. About six months ago, the tumor having enlarged materially, fluctuation could be easily detected on the left side. This fluctuation was connected with the tumor and not due to ascites. The nodular irregularities of the remaining portions of the tumor had not changed during the two

and a half years preceding. The woman's health was failing; feet and legs markedly swollen. Two months later I saw her in consultation with my friend, Dr. P. S. Conner. Abdominal enlargement materially increased, area of fluctuation also increased, lower extremities enormously swollen, with tendency to ulceration of the integuments of the skin.

Two weeks later Dr. Conner and I again saw her. Had lost strength, lower extremities still more swollen. Tumor seemed to become soft so as to present fluctuation at almost every point. There was now also clear evidence of fluid in the peritoneal cavity.

Uterine cervix could be detected by vaginal examination. Heretofore it had seemed obliterated.

Assisted by Dr. Conner, who fully concurred in the opinion that an operation was justifiable, and Dr. C. L. Bonifield, laparotomy was made at the date already named. We were of course prepared to remove the uterus, for we considered that the fibroid tumor had undergone cystic degeneration. On opening the abdominal cavity about one gallon of dirty grumous fluid was discharged from the peritoneal cavity. The tumor, as you will see, demonstrated by the section of its capsule which I here make, arose from the broad ligament. As you see it is of enormous size, but contains very little solid material. Its pedicle was about the size of my wrist and was easily secured. It had no uterine attachment.

There were some adhesions to the intestines and some to the omentum. The parietal peritoneum was everywhere extensively congested, and from a line level with the umbilicus to the bottom of the pelvis it was studded with papillomatous growths. In the bottom of the pelvis these papillomata were, many of them, as large as a hazel nut. Indeed, so numerous were they, and so large, that the lower pelvic excavation was at least one third filled. The outlook seemed not promising, although the patient stood the operation beautifully. Recovery, however, has

been rapid. Within one week after the operation the edematous condition of the lower extremities had completely subsided.

I can scarcely hope that this patient will escape malignant pelvic disease. I neglected to state that the drainage tube was kept in for five days.

The long duration of this tumor, the slowness of its growth, its solidity and irregular form, the mobility of the uterus with the tumor, the lifting of the uterus out of the pelvic cavity, uterine hemorrhage, though of slight degree, in a woman who had long passed the menopause, were not features calculated to lead to a diagnosis of a tumor of the broad ligament.

Case 2.—Here are two cysts one very large and somewhat solid, the other smaller. These were removed last month from a woman at Christ's Hospital. I was assisted in the operation by members of the house staff, Dr. J. M. Withrow, and my nephew, Dr. Bonifield.

Ether was administered by the interne, Dr.———. The patient was 49 years of age. The history of the abdominal enlargement was of four years' duration. Recently general health had failed.

I examined her for the first time ten days before the operation. Abdomen very large, symmetrical, fluctuation everywhere, intestinal resonance on the right side at upper portion of the pelvis, but none on the left. Vaginal examination disclosed the uterine os on a level with the upper portion of the pubic symphysis directed forward. Douglas's pouch filled with an apparently firm mass, certainly not fluctuating, possibly a little boggy. This mass apparently filled the posterior half of the pelvis, crowding the uterus forward and upward. Physical conditions, in this regard, indicate that the uterus was attached to this mass and resting upon it. Uterus immobile. Depth of cavity five inches. History of uterine hemorrhage.

Diagnosis: either fibro-cyst of the uterus, with ex-

tensive adhesions, or an intraligamentous cyst, so large, and so situated, as to be in very intimate relations with the uterus. Fibro cyst of the uterus regarded as most probable.

Laparotomy commenced with the expectation of having to remove the uterus. One half gallon of dark colored fluid found in peritoneal cavity. Parietal and visceral peritoneum deeply congested. A large multilocular tumor of the right ovary; pedicle about the size of my wrist, very long, which was found to be twisted four times around. Tumor extending over to the left side of the uterus, with such vascular supply, both from the uterus and the left broad ligament, as to make practically a second pedicle from the left side. This was transfixed and tied, after which the four twists were turned from the pedicle proper, when it was ligated and dropped.

It is very curious, and so impressed Drs. Withrow, Bonifield and myself at the time, that this extensive vascular and peritoneal attachment, on the left side, should have been established after the twisting of the pedicle. Unquestionably it was, for no twist could have occurred after its establishment. Again, it was remarkable that, notwithstanding the extensive twisting of the pedicle proper, the blood supply through it was in no way interfered with, both arteries and veins being large. On the left side likewise, there were very extensive adhesions between the tumor and the sigmoid flexure of the colon. So extensive were these, that the colon was bent around an angle of the tumor, and the walls of the cyst and the gut could be differentiated from each other with very great difficulty. There were likewise extensive adhesions between the greater omentum and the tumor.

After removal of this large tumor, this smaller one which I here present, and which is an ordinary ovarian cyst, though having very thick walls, was found crowded

so tightly into the bottom of the pelvis by the larger tumor, as to have caused it to present the immovable and firm conditions detected, upon vaginal examination, prior to the operation.

It was upon this smaller tumor that the uterus rested. Strange to say, there were no adhesions between it and the uterus or other structures. The patient has made an interrupted and perfect recovery.

This case likewise shows the difficulties and uncertainties of diagnosis in some cases of abdominal tumor, and for this reason I present it. I have stated that the patient has thoroughly recovered. So much cannot be said as to the operator. The two painful abscesses on my left forearm are the result of septic infection received during this operation. The tumor being very large, and the fluid in the interior cysts being too dense to be discharged through a Tait trocar, and the patient being in such condition that I regarded brevity, as to time of operation, important to her safety, I thrust my hand into the larger cyst and broke up the smaller ones, turning their contents out, thus receiving infection through a slight abrasion of the skin which had accidentally been made.

DISCUSSION.

Dr. E. I. Ricketts:

In regard to the specimens presented here by Dr. Reamy, I think that we are under obligations for their presentation. If I mistake not the sum and substance of the presentation of these specimens is as to the worth of a diagnosis previous to opening the abdomen. I don't know of any set of gentlemen that are always certain as to what is in the abdomen save those men that resort to electrolysis. They are the ones who always know just exactly what is on the inside previous to opening the abdomen. There was a time when I thought that in most of these cases I could say from the outside what was on the inside. After having seen such men as Thurston, Savage, Hamilton, Tait, and some others of the operators in these cases make diagnoses in the dif-

ferent clinics, and confess their inability to be accurate, I give it up. I have been asked by some of these men to take the patient in the adjoining room and make a diagnosis. I examined carefully, and came out, and was asked by Dr. Tait, "What is that case?"

"I would say it is abdominal tumor."

"Do you know what is on the inside?"

"No: I don't."

He says "I don't: we will cut in and see."

It has been my misfortune to think that I had a fibro-cystic tumor within the abdominal cavity. There was bleeding in this case, but not very much, and yet just above the symphysis pubis I could feel a hard, solid tumor, and just above that I got fluctuation; and yet, after I opened that abdomen, I found myoma of the uterus, with the ovarian pedicle twisted three and a half times. I know it was impossible to make a diagnosis of that case until the abdomen was opened. I have had the diagnosis of cancer of the liver made by gentlemen of the profession who are not excelled as general practitioners in diagnosis, and yet, when that abdomen was opened, I have found gall-stones. A year ago I saw a lady in consultation in Wellington, W. Va., that was suffering from a distended abdomen. This lady weighed 460 pounds, and yet there was no heart trouble, there was no trouble of the kidneys, and for the life of us we could not say from the outside the cause of this affliction. Having seen a case of this kind in which I myself was not able to make a diagnosis from the outside, I made an incision, and found a simple ovarian tumor; and it was on this account I urged exploration in this particular case. We cut in, and we found quite a quantity of fluid. The ovaries were normal; I couldn't satisfy myself as to what was the trouble. I received word last week that the patient had died, and on the post-mortem table there was found thirty-eight gall-stones in her bladder, and that was the only thing the doctor could find.

I know, as I said in the beginning, that we have some individuals that are always sure as to what is the matter on the inside; but they are the ones who open the abdomen less than some of those who do not make so many pretensions.

Dr. D. Tod Gilliam:

I will mention a case in which three different diagnoses

were made. This is one of those cases illustrative of the fact that we sometimes are unable to make a diagnosis until well into an operation, or until after the operation is completed, when, with the tumor in our hands, a scrutinizing examination reveals the true nature of the case. The patient, aged fifty-seven, had noticed the growth about five years ago, at which time it had attained to considerable size. Since then she had been treated by several physicians for abdominal dropsy, but never tapped. Finally, a few weeks since, Dr. Alderman, of Athens, being called to see the case, recognized its tumorous character and advised an operation. As the patient was greatly reduced and failing rapidly, arrangements were immediately made, and I was called to operate at her home in Athens County, Ohio. Arriving at the place and having only two hours between trains, I immediately made an examination while the preparations for operation were going on. I found the abdomen immensely distended and all the external signs of a cystic tumor. On making a vaginal examination, I found quite a large cystocele and the cervix crowded toward the pubes. The sound entered the uterus over five inches. By insinuating the finger between the cervix and pubes a hard, bony substance could be felt, which I could liken to nothing but the inferior maxillary bone of an infant. The anterior and upward displacement of the uterus suggested an intraligamentary growth, while the great depth of the uterus and the length of time which the disease had lasted suggested a fibrocystic growth of the uterus. I advised the patient of my doubts and of the probable difficulties and dangers in the way of an operation, but she insisted that we go on, and we soon had her on the table and etherized. On making the abdominal incision, I came upon what at first sight appeared to be a fibrocyst, as the tumor presented a dark-red and vascular appearance. Adhesions were universal and dense, and, after making some headway, I came to a place, about eight inches to the left of the median line, where I could not further effect separation from the abdominal walls. I now worked in various directions, and finally extended the incision upward. Here, to my surprise, I came upon the pearly, shining surface of the typical ovarian tumor, and was able to separate the thin, vascular capsule which embraced it as the ocular conjunctiva does the eye.

While little progress could be made by the fingers, I found that, after getting a start and making the parts tense by traction on the capsular membrane in one direction and the cyst wall in another, it could be quite readily peeled off by means of a sponge.

I was now quite sure that it was an intraligamentary cyst. But as I neared the base of the tumor the capsular membrane suddenly disappeared and I came upon a conglomeration of cysts, some of which were quite hard, and in the midst of these a well-formed pedicle. The tumor was removed and estimated at sixty-five pounds. The operation lasted forty-five minutes. In the confusion incident to the kaleidoscopic appearance of the cyst at different stages of the operation and the anxiety to catch the next train, I forgot to investigate the nature of the hard body felt behind the pubic bone *per vaginam*. As to the capsule of the tumor, I doubt not it was of inflammatory origin, but its remarkable vascularity and manner of distribution make it noteworthy. The patient recovered promptly, the highest pulse-rate being 86, and the temperature never reaching the fever point. I believe the older I grow the less I know about the intra-abdominal region, and the only way in which I am certain is to make an exploratory incision, and find out in that way.

Dr. Reamy:

I want to call attention to one fact that I did not fully bring out in presenting the cases that is of very great interest to me; and that is, that the last case that I reported, if you noticed it, is a case in which the pedicle was twisted four times. And yet that was a case in which the adhesion between the colon and the other side of the tumor existed, and likewise the adhesion of this same tumor to the broad ligament on the left side, the pedicle being on the right side; therefore, this twisting of the pedicle had occurred before this adhesion occurred. These adhesions could not have existed prior to the twisting of the pedicle, the pedicle twisted before these adhesions occurred. A very interesting question, connected with it, is, that notwithstanding this pedicle was twisted four times around, neither the venous or arterial circulation was interfered with. So, from the condition in which it was found, this pedicle must have been twisted six months or a year, maybe two or three years.

It was extremely interesting to note this adhesion on the left side, and under the point where the colon was in the tumor and adherent to it, and held down on account of it. It was the most interesting case in that regard that I have ever encountered, and the most difficult to explain.

Dr. Herrick:

I would like to ask Dr. Reamy if he regarded tumor No. 1 as of a fibroid character, indicating cystic degeneration at this point?

Dr. Reamy:

No. It is therefore very interesting in that regard. It was without any doubt a fibroid tumor, and it was myoma of the broad ligament that underwent cystic degeneration. It never had been an uterine tumor at all. Do you ask of the first case, or the last?

Dr. Herrick:

The first case.

Dr. Reamy:

The first was an ovarian tumor; the second was a myoma of the broad ligament.

Dr. Herrick:

I recollect a single case in which I was deceived in the same manner. The person presented the appearance of being about in the fourth month of gestation. I regarded it, and so gave an opinion, that it was a fibroid. She declined an operation. The case proceeded until she finally consulted other physicians. It proved to be a fully formed cystic tumor. It was afterwards removed. If I was to account for the hardness of it, and for the development of the dense firm structures which surrounded it, I would do it perhaps from the fact that it arose from the sub-inflammatory condition, which gave it that hardened appearance, finally created by the accumulation of fluid in the cystic tumor. This case was interesting to me in that connection.

Dr. Reamy:

One other object I had in reporting these cases is, that injustice is frequently done to honest and competent men in this very field. A man sees a case early in its life. The woman has a hard tumor. The uterus moves with it. The uterus is adherent, or ought to be; but not uteri

hemorrhage. Every man would say this is a fibroid tumor. He advises against an operation for the time. Time passes along, two, or three, or four, or five years, when this same woman gets into the hands of some other practitioner, who learns that history, and operates on the case, and demonstrates that it is not a fibroid tumor; and it goes all over the country, and doctors laugh about it, which they ought not to do, because the laymen are the ones that ought to be permitted to laugh at our blunders. They are not blunders: they are conditions which have changed, and from which no caution or intelligence can protect us. We don't know before we open the abdomen what all is in it; at the same time it is no argument that we ought not to make a diagnosis as near as we may.

HIGH AMPUTATION OF CERVIX—REPORT OF A CASE.

BY B. F. HART, M. D., MARIETTA.

In presenting the notes of the following case—"high amputation of cervix,"—I do not expect that there is anything new, *per se*, but the sequel of the case has been of much interest to me, as I have followed it step by step.

I was called July 13, 1885, to see Mrs. F. M., a strong, well developed German lady, aged thirty-four years, mother of eight children. She was flooding excessively; she had been troubled for the last three or four months in a similar manner, but not to the same extent. Had been under the care of another physician. Upon examination, I was satisfied that the case was malignant, confined mostly to the neck, resulting from an extensive laceration which had been without proper treatment.

I operated July 16. Used Sims' speculum, drew the organ well down, and with curved scissors clipped the tissue around, and well from the disease, up a little; then following the line up and inward on all sides, removed all that I thought necessary to reach beyond the disease. This left a funnel-shaped excavation in the organ with the apex upward. The hemorrhage at the first was very profuse, but plenty of *very hot* water, applied by a Davidson syringe, soon brought it under control. I then used an antiseptic tampon which was removed the second day; no hemorrhage followed; the surface was left to heal by granulation, the only other treatment being antiseptic irrigation. In six weeks she was able to do full

duty as maid-of-all-work connected with a large family.

I gave it as my opinion that the operation would stop child-bearing, but in this I was mistaken; at least to a certain extent. August 9, 1886, she had an abortion at three months. The waters broke some eight or nine days previous—recovery good. June 27, 1889, she was delivered of a seven and one-half or eight months' child. I saw her first on the 24th. The waters had been discharging for some days. She had some uneasiness through the abdomen, but not that of labor. The discharge was *very* offensive. I used antiseptic and disinfectant injections per vaginam, fearing sepsis. There being no pains of labor I gave chloroform, used forcible dilatation, and delivered by turning; child not much decomposed, but the odor of the discharge beggared description. The cicatrix was very firm and hard. Patient rallied well for three days; irrigation of vagina kept up; the uterus well washed out after delivery. The third day patient was taken with a very severe pain in left side at the lower lobe of left lung. Cough set in, expectoration profuse, of a dirty, ashy gray color; odor same as the uterine discharge. Were I to frame a name, I would call it septic gangrene of the lung. This cough and profuse expectoration lasted three weeks, attended by much prostration, and terminated quite abruptly in good recovery.

The patient has remained well since, except last year, 1890, she had glaucoma, for the relief of which she was operated upon by Dr J. W. Wright, of Columbus. She is at this time about seven months pregnant and enjoys good health. I would have been glad had her time of delivery passed so that I could at this time report the result, but as I am not in the abortion business the brethren will have to await further developments.

[The patient was confined on the 12th inst. Waters broke on the 8th, pain set in on the 10th. On the 11th there was still no dilatation. Gave chloral hydrate and morphia. On the 12th, patient became somewhat ex-

hausted, but still no dilatation. The os had the feeling of hardness with the feeling of wire, not dilated to the size of a ten cent piece. I then used forcible dilatation, as in the first labor, and applied the forceps at the brim and delivered a male child that weighed about seven pounds, and had been dead evidently for two or three days. The mother is doing well].—B. F. H., Marietta, O., July 14, 1891.

CARCINOMA: A FORM OF PERVERTED NUTRITION.

BY H. J. HERRICK, M. D., CLEVELAND.

In the mortuary reports of Cleveland, for five years, from 1885 to 1891, there were reported deaths under the head of constitutional diseases as follows: Cancer 452, marasmus or tabes 543, consumption 1,803, indicating a large ratio of deaths from cancer.

This statement alone is sufficient to arouse most ardent attention for the consideration of a form of disease so common and yet so uniformly fatal. In attempting to direct your attention to the subject of carcinoma, it is hardly necessary to more than allude to the arbitrary application of the name carcinoma, or cancer, as indicating no definite type of disease. No doubt this grouping in the report referred to includes a large variety of cases with no absolute unity in point of appearance or conditions, yet with certain characteristics in common. We may note in this grouping of diseases, also, that all are classified under the heading constitutional diseases.

I do not object to this grouping, but rather approve; for while I do not regard diseases as having such uniformity as to be capable of being classified like natural objects, I observe them grouped together, as being attributed to some common or constitutional cause. In my judgment they are at least first cousins in relationship, and all referable to some defect in some common or uncommon factor as essential elements of nutrition. Consumption is primarily and finally a disturbance of nutrition. Mar-

asmus is directly a result of defective, or entire absence of, nutrition, while carcinoma is a change in histological elements, owing to a change in some of the factors of nutrition. All malignant new formations, as also the benignant, are *growths*, and as such are dependent upon nutrition; of course the cell is to be regarded the element in all biological changes. All tumors of either class are atypical anatomically; but the carcinoma is atypical anatomically and physiologically, cells are abnormally large, diversified in form, with prominent nuclei and nucleoli; they have no uniformity as to density, and are held in a stroma of connective tissue, at one time firm and unyielding, with a dense connective tissue; at another sparsely distributed through the growth with large alveolar spaces packed with large, many-formed, rapidly proliferating cells, while in either form the other structures as nerves, vessels and lymphatics are in varying numbers, condition and influence. Indeed they must be looked upon as accessory parts, and as such determine much the character of the growth; specially is this true if we include the blood itself in the list of accessory tissue.

The point which I wish to emphasize here is that each variety of growth, natural or morbid, typical or atypical, are *growths*, and as such depend upon certain normal or abnormal conditions for their growth and variety of form and appearance histologically as well as anatomically.

If this is true, it is obvious that the departure from the normal must find its explanation in some change of some of the conditions of nutrition. It has long been a conviction with me, that all diseases are phases of perverted, or an arrest of, nutrition. We recognize nutrition as an elementary process, complex in itself, yet elementary so far as the tissues are concerned, and, owing to the obscurity of many of the minute factors, many give little heed to its importance in analyzing disease processes. If it is remembered that in nutrition we have a normal process, always uniform under uniform conditions, we gain cer-

tainly a more rational scientific view of disease. If we trace or seek to trace the departures from the normal, and seek to analyze the cause of the departure, and seek remedies that look to correct the disturbing influences, we are seeking in the line of accepted principles. It does not matter in this inquiry that we do not know essentially and minutely all the factors of nutrition; so far as we have knowledge it must run along these lines of inquiry. As our knowledge of the minute processes of life becomes extended and refined, we are able to follow the departure from the normal with more accurate reasoning with regard to cause and effect.

The electrician does not know the essence of the electrical force, nor all the laws of its action, nor the most efficient mechanism for using it, yet with certain known principles of the laws of its action he follows their lead. He is a scientific inquirer, and the art which he acquires is founded upon scientific principles. Looking upon disease as some modification of nutrition is both natural and scientific. In this view it must not be forgotten that all secretion and excretion may be regarded as analogous to nutrition in other parts.

We are all conscious of the fact that in the solution of any problem the results are determined very much by the premises with which we start. Objects seen by the eye assume, in our vision, different size and shape as they are seen from different points of view in relation with associated objects. In attempting the solution of any complex problem one must keep near to his principles and necessarily make use constantly of the elementary steps. The conclusions of the most intricate problems in logic find their support in simple self-evident statements. The conclusion of the most complex problems of mathematics are reached by use of the four elementary steps of addition, subtraction, multiplication and division; so in medicine, where we have perplexing complications with ever varying conditions and as a consequence ever vary-

ing phases of disease, it is incumbent upon us to always keep close to our elementary processes, if established.

"In simplici salis" we have a wise and safe motto, in attempting to solve the obscure problems of disease, and their treatment. If we attempt to interpret the varied and ever-changing symptoms of any disease, as seen through the complex arrangement of organs and their sympathetic relations, there will be manifest almost an infinite variety of disease phenomena which are referable to a common cause.

If—as is undoubtedly a common custom—we seek to find a certain disease by name, and in that connection seek the remedy for its cure, we are unavoidably committed to the idea that disease is something of a specific nature and due to a specific cause; not regarding it as due to defective organic function, we naturally seek some specific antidote.

The *germ theory* of disease which seems almost universally accepted would seem to lead in this direction, and when our most learned scientific observers and authors seek to give to the profession a specific remedy for a disease like consumption, so manifestly complex and of a constitutional origin, it is time the common sense of the profession should manifest itself. Let the idea prevail that bacteria in some of their varied forms are essential causes of disease, as seems to be the tendency to-day, we are driven to the necessity of seeking specific germicidal remedies for the supposed specific disease. We have left simple elementary principles which are under our control, and are afloat on a broad, illimitable sea of speculation and empiricism. In all the insane search after the cause of disease we are not able to point to one single one of the so-called germ diseases due to a specific microbe and say here is the germ and here is the antidote.

Disease is not an entity having specific characteristics. It is the result of a disturbance of a normal process. Natural objects have such uniformity as to give a basis for uni-

form classification, diseases or abnormal processes have not. A disease must be traced to its anatomical source and the particular organ affected, and symptoms discriminated from the disease and referred to their prime cause. We all believe that anatomy and physiology contain the only substantial lines of inquiry with regard to disease. Nutrition is a defined and accepted process in physiology—its conditions are defined and definite. If nutrition is perfect no disease is present; if not, there is disease or a tendency to it. All disease therefore may be regarded, as we have said, as a phase of disturbed or arrested nutrition. It is not my purpose to follow this line of thought further, as I have so far presented the relation of disease to the physiological processes, that we may appreciate the application in attempting to solve the problem before us as to the nature of carcinoma.

All new formations are but growths and, as such, are produced by the factors of nutrition. The tissues of the new formation may conform histologically to certain normal tissues and are called homologous; or differ in size, shape, etc., from any normal tissue, and hence styled heterologous. The former are called typical with reference to the normal parts, while the latter are regarded as atypical; terms which imply what we recognize as a fact, that all tissues as well as every member have their own peculiar type. Knowing something of the conditions and processes of the formation of the typical tissue, our problem consists in analyzing them, and if possible determine how those conditions are changed to form the atypical.

By the aid of the microscope histologists tell us that these growths are formed of cells of the epithelial type, densely packed in the alveoli of a fibroid stroma which is more or less dense. The cells are larger than normal and diversified in form, having prominent nuclei and nucleoli.

It is these peculiarities alone which indicate to the microscopist the cancer cell as distinguished from the normal.

There is no specific cancer cell. It is judged cancer from its size, shape, nuclei, etc. The cells are atypical, as the whole growth is. They take an exuberant growth without becoming developed into normal tissue. They may be regard as a form of degeneration; they grow rapidly with defective development. The stroma also which forms the alveoli is of a fibroid structure, variable in quantity and quality. In the variety known as encephaloid, or soft cancer, the stroma is scant, the fibrous filaments are few in number and weak in structure, while the cells are large and not developed.

In the scirrhus there is a large amount of connective tissue stroma with small compact alveoli densely packed with cells. There are also in these new formations the other essential structures for growth and vital connection with the whole body, viz., nerves, arteries, veins, blood and lymphatics. These are to be regarded as accessory to the growth. In the processes carried on through them may be traced the peculiarities of the disease. All tumors, as all tissues, owe their growth to the processes carried on through these agencies, the cell being the histological element. Cornil and Ranvier have shown that lymphatics communicate freely with the alveoli, and in this we are able to understand the tendency of cancer to affect adjacent lymphatic glands and through them the whole system.

The fluids from a heterologous growth may be supposed to be abnormal and are returned to the systemic circulation through the lymphatics, and, being abnormal, work their pernicious effect upon the glands first, from which, being carried into the veins, their second effects may be traced to the liver, lungs or kidneys, organs of elimination, excretion or assimilation. Thus it seems we are able intelligently to appreciate the infection of the general system through or by the morbid growth. Indeed every case, in its onward march to a fatal issue, can be quite definitely traced in its course through these processes.

Recently a case came under my care in which I was

able in a marked degree to observe the results of this transition of the disease, from the part first affected, to the internal organs and cause death.

Mrs. C., aged 64, came under my care the 19th of September last, with the following history:

Six or seven years ago she discovered a lump in her right breast. Little or no attention having been given to it until three years ago last July, then a papilliform growth, the size of a walnut, as she expressed it, had appeared on the nipple of the same breast, also axillary glands were involved. At that time the surgeon consulted removed the growth on the nipple with a ligature, giving no attention to constitutional condition. The 28th of November following, he removed the breast and axillary glands. The wound healed well, and for two years there was no appearance of return. Early last spring the growth made its appearance again in the cicatrix and in axilla, accompanied with severe pain. July last, a second removal was made by the knife. No medicine was given and no attention given to constitutional condition as I was informed. In August a severe chill occurred, accompanied with sallowness and general discomfort. Five weeks afterwards a second chill occurred, at which time I was called to minister to her comfort. At that time, September 19th, there was fever, very deep icterus, tenderness over hepatic region, nausea, anorexia, clay colored stools secured by cathartics. At this time, local and constitutional means were applied to relieve the condition, but with no avail.

The cancerous growth had appeared again in cicatrix of old wound which was treated with the effect of arresting its growth. I administered active corrective treatment, followed by Donovan's solution, and at the same time applied to growth an arsenical plaster, which caused a black slough, which in due time came away followed by no tendency to return but rather to heal. Meantime the disease of the liver continued. She died September 28th.

An autopsy showed the whole liver to have suffered from the cancerous infiltration. The gall bladder had shrunken to be hardly recognizable, as the entire secreting power of the organ had been lost. The organ was large, perhaps one-half larger than normal, very dark color, friable, capsule adherent; the deep color appeared like that of cholesterin and deep bile pigment. Other organs were normal.

This case illustrates the course of the disease as the system becomes infected by absorbed juice from the first degenerate abnormal growth. The lymphatic vessels and glands were unable to receive and transmit the abnormal fluid to the general blood current, hence the involvement of the adjacent glands; subsequently, the same morbid material being carried to the systemic circulation must be assimilated or eliminated, but assimilation being impossible, it, in the effort of the system to eliminate through the organs of excretion, works its destructive effect upon them.

By tracing the course of the infection received through lymphatics or veins, by whichever course it takes, we are able to trace the disturbed physiological processes after the first formation. The internal and vital organs first suffer on account of their inability to carry on their function for the elimination of the poison. In the course of the infection the first obstacle met is in the liver, the disturbance of which accounts for the peculiar mahogany, sallow complexion, which indicates the cancer cachexia. We can all recall cases which illustrate this course of the disease as explained by this process.

Recognizing the first appearance of the new formation, as a formed degenerate tissue, due to a depraved condition of system and defective nutrition, the question of vital moment is as to the disturbed element of nutrition which causes this inferior tissue. By many the disease is thought to be local, and if early removed, before it has extended its pernicious influence to the system, the best chances are given to the patient.

If we start upon this plan of treatment without regard to the prime cause of the morbid growth, what, if any, guaranty can we promise that the disease will not return again and even more rapidly where the defective elements of neoplastic growth appear for the repair of the injury caused by the removal.

It has been my experience and observation substantiated by a large number of cases that early attention to the constitutional condition, securing a high degree of vital activity preceding and subsequent to removal, is of the greatest importance. The surgeon is anxious with his knife to eradicate every vestige of the new growth. It appeals to his skill, and the importance of this procedure is urged, but if he has in mind the antecedent cause he will wisely counsel with the physician or, if competent, with the constitutional condition which was the original cause of the disease.

Nature is jealous of her rights; she would have the physician and surgeon assist where she is lacking. They must always bear in mind that the minute processes of nutrition are to be their allies, whereas there often seems to be a controversy between them; there is the local, versus the constitutional, treatment; the *disease must be cut out.*

As a rule, specialists magnify the local or the organ, and forget that it is a part of a whole organism which must thrive or suffer through the well-being of all. Does the experience of the profession sustain the idea of removal of cancer alone? It has been to me a matter of no little wonder that a disease of such universal fatality and yet so common has been so little understood and controlled.

Indeed, we may confidently claim that no system of treatment to-day excels by any considerable extent that commended by the father of medicine, Hippocrates, over two thousand years ago. In his aphorisms we find quite as safe, cautious and successful treatment proposed as is

found in most standard authors of to-day. He says "it is better not to apply any treatment in cases of occult cancer, for if treated the patients die quickly, but if not treated they hold out for a long time." Is it not true that this is good caution for us to-day?

Regarding the obscure questions as to cause of cancer, I desire a word. We are taught that it of all diseases bears the marks of inheritance, and if among near or remote ancestors any form of malignant growth, or what has been vaguely called cancer, has appeared, the present and succeeding generations are liable to have entailed upon them the seeds of that malady. Many families to-day are living in terror lest some of the hapless *germs* shall strike them. Is it not time that the profession had dropped some of its vagaries and placed itself upon the basis of following the lead of our elementary principles? No one supposes we inherit specific diseases, but rather constitutional conditions—dyscrasias. Every organism has its type as an entire body, as also in its several parts. If that type is not secured and maintained it is because some defect exists in the conditions of nutrition. Morbid types existing, if removed, there is a disposition under the law of our being to revert to the ideal type. If we can always have in mind the two conditions, the ideal and the real, we do well. The important questions are, if the ideal is not attained, why not and what can we do to promote it?

Here let me say that we are unable by any refinement of the microscope or minute chemical examination of the blood, nerves or tissues to point out the "materies morbi" whether it be specific or some inconstant disturbing agent; but we may be assured that the fine adjustment of the different tissues and the complex conditions of nutrition will bring to view any abnormal conditions. By holding in mind steadily the conditions of the normal, we may be able to perceive and perhaps analyze the causes of the abnormal.

Here we reach the point where different schools of pathol-

ogy of different periods in medicine diverge. There have been the solidists, the humoralists, neuro-pathologists, and now germ pathologists. All diseases now are said to be caused by minute organisms. Not to enter any field of controversy, we know that the blood is the mother of all tissues, normal or abnormal. We may further declare that the repair of all tissue and the growth of all new tissue finds its elemental structure in the leucocytes of the blood.

I am aware of the fact that in this line of our thought, we get back to the humoral pathology of Galen, now regarded as obsolete. The blood to be sure, is a dependent fluid, as Virchow has shown, but it is specially dependent upon well recognized processes of secretion, digestion and assimilation; processes the most complex, yet on which we depend momentarily for transforming the varied and complex articles of food into blood.

Billroth says, referring to the different classes of new formations, "we must here add that there is every probability that wandering cells escaping from the vessels very materially aid in the formation of tumors." "We find ourselves here," he further says, "in the same position as in inflammation, most possibly all of the indifferent cells are white blood cells now among the tissues as wandering cells."

He further declares it in the highest degree probable that a large part of the young cells, escaping so extensively into the tissues during the development of tumors, are wandering white blood cells, and these, endowed with nascent life, develop into new tissue allied to that in which it is located. Still further the character of the growth, as we may readily infer, is modified by the condition of the cell itself, as also that of the entire blood plasma, cells, salts and minute undefined granular matter. Now by this view of the genesis of the growth of tumors, we may find our theory illustrated and confirmed by the facts, that tumors are prone to succeed injuries which would result in inflammation or departures from the normal.

Approaching our subject with this view also we may readily conceive of the growth of heterologous new formations, as due to certain abnormal states of the blood.

As heterologous growths are prone to assume the rôle of so-called carcinoma, or malignant growths, we may observe that the state of blood is the factor of first importance in any attempt at solving the problem of removal or treatment. To state the theory then drawn from this course of reasoning, which seems sustained by high authority and also good sense, it is, 1st, that all new formations are formed from an exudate including leucocytes, which in their nascent state form the germ for the new growths. 2d, the entire character of the new growth as to benignancy or malignancy is determined by the character of the exudate, which is determined by the constitutional condition of the subject. The subject may have acquired or inherited a dyscrasia or vice—call it what you please—which directed the growth into its atypical form.

The new growth becomes excessive in growth but defective in development; it is not up to the standard of any normal typical tissue. It is a degenerate tissue because of its atypical degenerate character; its detritus or juices, the result of metabolic changes, are unnatural, and being thus carried into the blood current by the lymphatics are to be excreted; but being unnatural cannot be eliminated, hence work their disturbing influence upon the organs of excretion. In the case reported, the liver, the chief organ of excretion and assimilation, became affected. This in brief is a theory that has rested upon my mind as offering the most rational view of the disease and opening the most rational line of treatment. With this theory, the constitutional condition is primary in causation; and I further believe that the prime factor in the morbid process is to be found in the condition of the blood. This I am aware savors of the old humoral pathology, but it seems to harmonize with our physiological teaching. The blood is the life of the tissues and in-

deed of all physiological processes. Further confirmation of this theory is found in certain facts:

1st. Neoplasms assume the type of the benign or malign owing to location and certain conditions of blood not specifically defined but due to inappropriate food or some defect in its transformation through defective secretion and digestion.

2d. Traumatism is quite frequently succeeded by new formations.

3d. Those of the epithelial type are prone to occur in those parts, which, from exposure to recurring and constant irritations have an extra determination of blood to meet the increased demand, as in the mammæ, pylorus, lips and tongue of smokers and tobacco chewers.

4th. While age in years is not essentially a predisposing cause, yet, owing to the fact that vital activities are generally lowered as the zenith of life is past, there is in new growths, in advanced age, as in the normal parts, a greater predisposition to take on a degenerate form—heterologous tissue, carcinoma.

From these considerations, if well founded, it is obvious that the treatment which promises the most success, rationally considered, would look first and constantly to the constitutional condition which disposed to the first departure from the normal.

Second, to the local treatment which looks to the change or entire destruction of the new formation.

Following these conclusions the local treatment may be briefly stated as follows:

1st. Complete extirpation with the knife of both the primary growth and infected adjacent glands. This we must regard as never justifiable until after such constitutional treatment has been pursued as to correct any derangement of blood or impairment of blood-making or blood-purifying organs. For meeting this demand, medication with other means, as exercise, general constitutional care and food must be looked upon as requiring most careful consideration.

2d. Destruction by caustic and alterative escharotics; the same antecedent constitutional care being required for removal by these means as by the knife. The agents commonly used for this purpose are plasters of arsenic or chloride of zinc. Professional experience gives preference to the arsenic. My own experience has been such as to strongly sustain this choice. Indeed it seems to me that the use of arsenic for this purpose has not received the credit to which it is entitled. It is quite remarkable to observe how rapidly and kindly the ulcer will heal after the slough which follows the use of the arsenical plaster.

3d. By hypodermic injections. In my experience I have found that treatment by this procedure may successfully be adopted, where the new growth has occurred in parts where removal by the knife or caustic is not admissible, as in the antrum—certain bones, throat and uterus.

The indications are to use some substance, which will arrest the rapid cell growth, destroy the growth already formed and favor resolution.

In my experience the substance which meets these incations with remarkable premise is bichloride of mercury 1 to 3000 or 5000.

Not to prolong this paper, already too long, I may conclude with the personal conviction drawn from reports of cases, records, and personal experience, that surgeons are specially prone to overestimate their power to cure this form of disease by local means alone.

DISCUSSION.

Dr. D. N. Kinsman:

The paper of the distinguished author merits discussion, as much from what he says as from what he does not say. I do not believe in the constitutional origin of carcinoma.

Chimney sweepers' cancer comes from local irritation.

Smokers' cancer of the lips comes from local irritation. Cancer has developed on the side of the tongue evidently dependent upon irritation of that organ by a jagged tooth.

Briesky says, the persistent irritation of a lacerated cervix uteri, by friction on the surrounding tissues, causes the development of uterine cancer.

These are all cancerous developments; glandular enlargement and constitutional infection come later.

What is a cancer? Waldeyer says it is an atypical epithelial neoplasm. When we study the intimate structure of a cancer, we find it is a peculiar arrangement of stroma, from the middle germinal layer and "nests of cells" contained therein, which come from the superficial germinal layer, and upon the relative proportion of these two elements we build our classification of carcinomatous tumors. Scirrhus, encephaloid or colloid are terms which are used to describe the appearances, to sight and touch—as hard, brain-like, or jelly-like.

The downward growth of epithelia into the subjacent structure makes a cancer, and to me it is a matter of wonder that cancers do not more frequently appear, as the result of violence applied to what are known as "seed warts." This sometimes occurs, and we have a local development.

How do these cancers spread? Not in the juices which flow through the growth, and thence into the sublying territories, but by outgrowths from the neoplasm. I have many times examined sections of tissue taken from the borders of mammary cancers, and therein have observed little colored lines of cells extending from a large nest out into the surrounding tissue. There they establish a depot of new cells, and thence a new line is thrown out, as the engineer builds a cantilever bridge. All around the central neoplasm these networks radiate, until an entire gland, as the breast or testicle, is involved. All this takes place before any so called cachexia appears.

Whatever be the initial cause of this "rebellion of cells," whether from inclusion of "islets of embryonic tissue," which have been stimulated to baleful energy, as cancers of later life, or from parasites, the "aniline bodies" or other organisms, the beginnings are local.

The permanent cure of cancer by surgical proceedings, is proof of this statement. Those organs most subject to

traumatisms, the mammary glands, uterus, testicles, etc., are most frequently the seat of cancerous growths.

The secondary or constitutional symptoms arise from mechanical disturbances of nutrition, from the wearing effects of pain, and from septic conditions dependent upon ulceration. Measures constitutional or otherwise are demanded in the treatment of these conditions, when they exist, but no constitutional treatment ever dreamed of or applied, so far as we at present know, will prevent or cure carcinoma.

Dr. C. E. Beardsley:

The doctor had a very able paper, but it did seem to me that he had commenced at the very bottom — at the cell. I believe that the whole body of cells or the organ is only a cluster of cells so peculiarly put together as to constitute an organ. When a cell becomes diseased, it matters not how, from irritation, it sets up an action, a growth, and this growth passes on into the body in which it belongs. Whether it be in the liver, the lung, or in the parotid gland, or in the epithelium of the lip or tongue, it constitutes a disease—a tumor—carcinoma of the organ, that is peculiar to the cell that is in that gland. In speaking of the cell, he gave the pathology, the anatomy, in splendid style; but in this minutia, in differentiating the different classes of cells, either in disease or in health, as Prof. Kinsman stated, he never mentioned, as I could discover, the crucial test—that is, the stain. We know that the parotid gland cell is entirely different from the liver. The cells of the kidney stain differently. It seems to me it is the crucial test, not only in differentiating the cancer cell, but in differentiating among the normal physiological cells. It seems to me that we should seek a remedy (which he never spoke of in his paper). That is what we are to seek for: something to cure the disease. Now it seems to me that each cell or each body of cells constituting an organ has its affinities, and they make no mistakes in their selection of material to build themselves up. The bone cell always selects the material necessary to build up the bone; even the enamel of the tooth selects its proper material. And they never make any mistakes. And when we have this abnormal growth present, a cancer, we should select a remedy which will assist the organ affected in the selection of it

proper material. But the thing I want to bring before the society is, that you will notice that the same classes of cells in different tissues stain differently.

Dr. M. Stamm:

I think the view submitted by Dr. Kinsman is accepted by almost every pathologist of today, and it ought to be by every physician, I think. It matters little whether cancer is due to a specific thing or condition of cells; the only thing is the local or constitutional effect, primarily. I think all modern pathologists accept the view that it is a local affection. With regard to a cure, constitutional measures have been tried for ages without any success; but since the view has been adopted that cancer is a local disease primarily, physicians have resorted to the knife more frequently. Within the last two or three years, since the death of the Emperor Frederick, Virchow has investigated the matter again, and he has come to the same conclusion that he had before, and he especially advises an operation in cancer as early as possible. Now the results seem to encourage the surgeons to do that. He reports three cases of operation for cancer of the rectum, in '73, '74 and '75, and they are living yet. The most important thing is to do it before the lymphatic vessels or cells are infiltrated; and if you have any suspicion of an infiltration of neighboring cells, remove them. Since surgeons have practiced the extirpation of the uterus for cancer, the results have been very encouraging, patients living for eight or nine years.

Dr. Herrick:

Gentlemen, I desire to express gratification on account of the free discussion of the views presented in my paper. I attempted to show that all cancer growths are to be looked upon as a form of perverted nutrition and that in this line of inquiry we have the satisfaction of knowing that we are seeking in the line of accepted physiological science.

One gentleman alludes to the cell as the histological element that is to be inquired of in settling the problem. This I endeavored to distinctly maintain in the views presented. The defective nutrition must find its cause in imperfect cell growth or development, owing to defective cell imbibition. I further sought to show that the new growth finds its exciting cause in some previous injury or

prolonged irritation of a part being predisposed to by age or failing powers of patient, owing to defects in blood. The leucocyte is the nascent agent of the new growth, being the element in the exudate destined for maintaining or repairing the injury or waste of the suffering tissue.

The new tissue formed is typical or atypical according to the normal or changed condition of blood. In this line of reasoning, I think we have clinical facts and scientific principles to sustain us. Admitting this, the line of treatment most essential consists not alone in an effort to remove every vestige of the new growth, but primarily and constantly looking to the constitutional condition which first predisposed to the atypical growth.

I present these thoughts not as entirely new, but in my humble judgment as rational and offering most promising hope of relief to a class of sufferers from a most unrelenting and fatal disease.

In addition to the jacket, plaster suspenders are made by throwing three or four folds of the bandage back and forward over the shoulders. The use of these suspender casts of the shoulders will be described later. The jacket is removed in the ordinary way by cutting down over the sternum, but the shoulder straps are not cut, but are drawn over the shoulders as a vest is taken off.

Instead of the jury mast of Sayre, or the spring traction collar of Roberts used where the spondylitis is in the dorsal or cervical vertebra, a substitute has been provided which is made by making malleable steel suspenders after the form of the plaster suspenders above referred to. These are formed over the shoulders, one on either side, and are continuous with the upper ends of the traction bars of the jacket.

From the highest point on these suspenders or about under the ears a " C " shaped piece is riveted which curves towards the neck and around the coat collar to a point directly over the other end. From this point another set of extension bars passes up along the side of the head superior to the parietal eminence. From these the ordinary occipito-mentum suspension is secured.

No one has ever claimed that the plaster of Paris jacket retains that degree of tension made upon the spine at the time the body of the sufferer is suspended by the tripod, and as a matter of fact the body after a day or two conforms to its right encasement about it and settles down so that all the extension made at the time of adjustment is lost, and as time passes the plaster breaks and gives way and the thorax continues to slide down so that in a short time after the plaster jacket is applied, the amount of traction desired by the surgeon, and obtained by him at first, is materially reduced, and the jacket remains as a support only, and pain at the point of the disease again appears and another jacket is necessary. We can at least prove that the extension powers of the plaster jacket are daily diminished instead of increased.

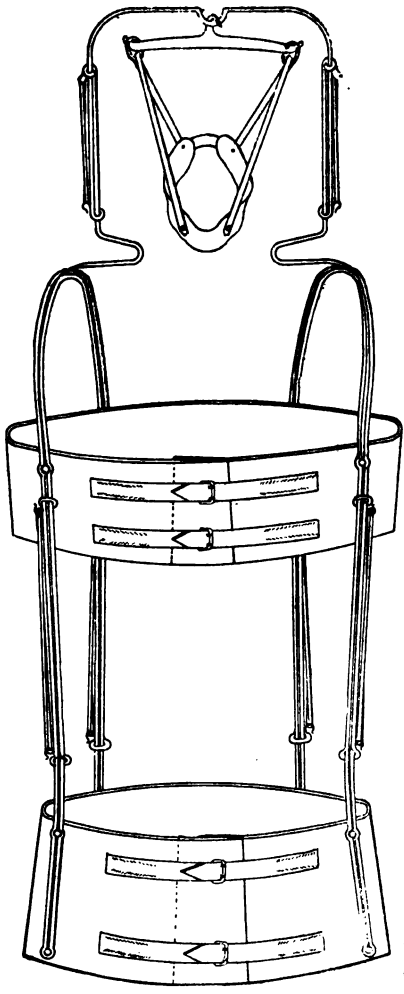
I am aware that many of the orthopædic surgeons of to-day do not believe in traction in the treatment of any form of joint trouble, but insist that such treatment is positively injurious, but as for myself, I have other testimony, and believe these cases receive the most benefit from a spinal support that not only retains that degree of traction made upon the vertebral column at the time of the application of the support, but will, if desired by the surgeon, increase the tension at will.

Great traction is seldom demanded, but all cases do demand that the weight of the body should be kept off the diseased area during the stage of molecular disintegration, absorption and reproduction of the vertebral body. This can not be expected if the body is pulled up by a tripod for a day or so and allowed to sink down into a jacket, the jacket then cut open, removed and re-applied or renewed.

This trouble admittedly begins primarily as other joint troubles, as an inflammation of the synovial membrane between the vertebral bodies or as a central osteo-myelitis in Pott's disease and as inflammation of a lateral facet either as a synovitis, or ostitis in scoliosis, or lateral curvature, or distortion. Unless this inflammation is controlled very early it advances as does hip and knee cases, to an involvement of cartilage and bone successively, and a distraction of the vertebral continuity is the result, and the various deformities, viz. scoliosis, lordosis, gomphosis, etc., necessarily follow.

I want right here to refer to a few points in diagnosis, not for the benefit of the orthopædic specialist, but for the general practitioner, under whose observation these cases almost invariably first appear.

Incipient synovitis of the lumbar spine is too frequently called by the vague term "lumbago," and early spondylitis of the other regions is generally put upon treatment for congestion or anæmia of the cord, etc., and the true malady allowed to go on uninterrupted; the true nature



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of the trouble being discovered only when the unmistakable symptom of spinal distortion occurs, when it is, alas! too late to cut short or abort these terrible afflictions.

The most important diagnostic procedure is, after first locating a painful point, either made so by pressure or constantly painful, to gently tap either shoulder with the side of your hand. If the spine be diseased the pain will be increased. Now grasp the patient under the arms and lift the trunk's weight off the pelvis with sufficient force to relieve its weight from the spine, and if the spine be the seat of synovial disease the pain will disappear. Now sway the body from side to side, and if the synovial membrane of either lateral articulation be the seat of the trouble the pain will be relieved when the body is thrown from the trouble, and increased when bent towards it. Again ask the patient to walk across the floor and observe his gait and carriage. If the spine be the seat of the trouble, he will be found to walk cautiously and with a gentle tread and not on the heels, for forcible locomotor treads impart to the diseased portion vibratory concussion and pain is increased.

These spinal troubles are very often the result of spinal sprains. You can not always expect to find pain in the early stages, and not infrequently do we meet cases without a history preceding deformity.

In the treatment, the first indication, I believe, is to relieve thoracic pressure upon the inflamed synovial membrane and give the diseased parts an opportunity to repair, or if it has gone too far, or was primarily an ostitis, it can pass through the various stages with a minimum of deformity and positive assurance that the deformity will be less, rather than greater.

This jacket is of great value not only in lateral curvature and lordosis. By placing the extension bars in opposition to the line of deformity, the distortion can be gradually and continuously overcome. To illustrate, in cases of lordosis where it is desired to have the fixed point as far forward as possible in order to throw the trunk weight back, as well as up, the lower ends of the two bars can be secured to the lower segment of the jacket, anterior and superior to the anterior superior spine of the ilium and made to curve around backward and upward to a point between the posterior axillary line and the inferior angle of the scapula; the direction of the bars in all cases to be so arranged as to be in opposition to the line of deformity.

If there is anything in suspension in the treatment of diseases of the spinal cord as practiced by S. Weir Mitchell in cord troubles secondary to vertebral diseases, and by Charcot and others in locomotor ataxia, the same end can be obtained with this suspension jacket.

The jacket affords, first, a permanent structure; second, support that increases if desired and never decreases; third, the most comfortable of jackets; fourth, can be removed at any time and readjusted without destroying the jacket or its supporting powers.

Jury mast affords, first, continuous elevation of the head; second, freedom of movements of head; third, pa-

tient can lie down on the back, thus affording one of nature's greatest remedies, viz., recumbency; fourth, never necessary to cut the clothing, for the brace fits the coat collar; fifth, suspension from four different points, equal distance about the pelvis, and not from one point over the vertebra which increases pressure upon the diseased area.

These braces are all made of wire, and a twofold advantage will be found in the use of this material for their construction: first, economy; second, a minimum of friction.

The greatest objection made against this mode of mechanical therapeutics is that it would not work because the friction of the bars almost equaled the extension powers of all the elastics that could be used.

With the wire braces, however, there is no friction except at the two bands which hold the bars together, and that instead of being a flat surface is merely a circle in contact with another circle, which as you see affords a minimum of friction. Besides, others who have practiced elastic traction, have always used the elastic webbing. For great distances this may do, but the space used at either end with leather, etc., to make the elastics so that they can be attached to the bars, destroys the usefulness of the elastics at least fifty per cent. I always use the rubber rings of Faber, etc., and I can get all the power necessary to overcome the contraction of any set of muscles.

Cut No. 1 represents the jacket to be used in lumbar dorsal disease.

Cut No. 2, with the jury mast is to be used in cases where the caries is above the eighth dorsal vertebra or in cervical caries.

The jacket is not made solid because it is not necessary, but in cases of the upper dorsal, say from eighth to third, both the jury mast and complete jacket should be used to insure the best possible results.

In cases where the caries is in the cervical region,

especially the upper portion, the lower extension bars, or those about the thorax, are unnecessary, for the ones from the shoulder suspenders up have sufficient power to support the head.

INTUBATION.

BY GEO. GOODHUE, M. D., DAYTON.

The idea of relieving laryngeal stenosis by some mechanical contrivance, introduced through the mouth, was at least considered as early as the beginning of the present century. Catheters were introduced and temporary relief afforded, but of course little or no permanent good was accomplished except to draw the minds of inventive genius to this field of work.

The first tube placed and left wholly within the larynx, so far as history tells us, was done by Bouchut, of Paris, in 1858. This was a cylindrical tube and so short that it scarcely reached below the glottis. A thread was left attached in order to enable the operator to remove it. Seven cases were thus operated on, and with the aid of tracheotomy two lives were saved. Just what purpose tracheotomy subserved in assisting intubation in these two cases I cannot understand.

A committee was appointed by the Paris Academy of Medicine to investigate this innovation and ascertain whether or not there lay any germ of truth or value in the claims of this physician, who had the courage to laud his discovery and condemn tracheotomy so generally practiced. As was natural some of the most eminent tracheotomists were appointed on this committee, and their report was so bitter and thoroughly condemnatory that it not only prevented further inquiry being made in this field of study, but even disheartened the inventor and caused him to abandon it altogether. For nearly a quarter of a century intubation slept, and so sound was

its sleep that it was some time after Dr. O'Dwyer commenced his experiments with intubation that he was aware that another had had his rise and fall, while battling in the same field. In 1880 he began intubating, but almost wholly in hospital practice among the illy fed and puny children of the city poor. His results were far from encouraging, and he undoubtedly would have abandoned it had he not observed that tracheotomy performed on the same class of patients yielded no better results. His tubes were short like those of his predecessor, but unlike them in being bivalve. Instead of leaving a thread attached, there was a wire on inside of tube, by means of which he was enabled to hook it out. The head of his tube was very small, which permitted it often to rest upon the true vocal cords. He soon, however, enlarged the head, changed the bivalve to an elliptical tube so that undue pressure would not be exerted on vocal cords, lengthened his tube, and enlarged it at its middle to favor its better retention. This is the tube most generally used to-day. Some modifications have been made, the most notable of which is perhaps that of Waxham's metal epiglottis to facilitate swallowing of liquids, but even this is of doubtful utility. With the same end in view J. Mount Bleyer has constructed a tube with extremities made of hard rubber and central portion of soft rubber, and claims much for his modification. To Waxham belongs great credit for venturing to use intubation in private practice, and his report of eight successful results in seventeen successive cases did much in popularizing it and bringing it into general use, so that to-day it is recognized as a justifiable operation in almost all civilized lands. Theoretically this operation seems as good as any, and actually in our experience is it of value, is the question for us as practical physicians to decide. I report my cases, not because I have had better success than others, but simply to keep prominent before the medical fraternity an operation that yet in its mere youth—hardly ten years of

age—is already vieing with that old and valuable operation, tracheotomy.

Case I.— Intubated March 29, 1889, 4 P. M. Age two and one half years. I was called to see patient at noon, March 29, 1889, and found him suffering from nasal, pharyngeal and laryngeal diphtheria. He had suffered from difficulty of breathing only about 24 hours; but the nasal and pharyngeal trouble had existed for four or five days. The struggle for air was most distressing. He had not slept one moment during the previous night.

Although the hope of a successful operation was very slight, I advised intubation, which was readily accepted by the parents. I made my first attempt, and was convinced that it was much easier for a novice to introduce a tube into the œsophagus than into the larynx. In the fourth endeavor I succeeded in reaching the larynx with my tube, and the effect produced upon the little fellow was marvelous. He took liquid nourishment quite well, and had almost no difficulty in respiration until his death thirty hours afterward. The comfort afforded by the tube was so great that the parents told me that had he lived only a half hour after operation, they should have been glad that it had been done. Death was caused by diphtheritic toxæmia.

Case II.—July 11, 1889. Age two years. Intubated in the morning with complete relief of dyspnœa. Case seemed promising. Summoned hastily in the afternoon by Dr. J. A. Romspert, the attending physician, on account of return of difficult breathing. Thinking the tube might be partially clogged by membrane I prepared to remove the tube, but found none. Concluding that it had been ejected and swallowed I introduced another, but as it gave very little benefit I removed it.

Child died late in the afternoon of same day, from extension downward of membrane.

From insinuations made by an unprincipled brother practitioner the family concluded that by introducing

my second tube I had driven my first tube into the lungs and had killed the child. A malpractice case was threatened, but an autopsy, revealing the duodenum as the resting place of my tube, averted further trouble.

Case III.—Intubated October 12, 1889. Age seven years. Courtesy of Dr. H. S. Kimmel. Present, Dr. J. M. Weaver. Patient had been sick five days. Had seen him twice in consultation, found him breathing with difficulty and gradually growing worse. Advised intubation, which was declined. Finally the case became so desperate that they could see that only a few hours more of life could be hoped for without mechanical aid, and consented. His lips were blue, and large drops of perspiration covered his face. Tube was easily introduced and great relief afforded. Coughing, however, was almost incessant, and several large towels were saturated with mucus and softened membrane. In about ten minutes, tube and quite a large quantity of membrane were ejected. Difficult breathing, however, returned immediately, and although his coughing had been violent, the relief he experienced had been so great that he waited with impatience the preparations necessary for re-introduction. After this, he had very little difficulty in breathing, and took liquid nourishment fairly well.

He had pneumonia at time of operation. Soon after, his temperature ran up to 103°. Respiration between 50 and 60 for several days. Tube removed on fifth day. Convalescence slow but uninterrupted.

Case IV.—November 4, 1889. Age four years. Courtesy of Dr. Shepherd. Drs. J. S. Beck and P. N. Adams present. Called in great hurry. Demand for intubation extreme. Easily performed—relief instantaneous and complete. In a few hours began to breathe with some difficulty, which increased until death two days later from extension of membrane.

Case V.—November 4, 1889. Courtesy of Dr. Shepherd. Age seven years. Drs. Adams and Beck present.

Two other children in family sick with diphtheria at the same time. Had been sick about two days. Had had several paroxysms of a frightful nature the previous night from which they did not expect her to recover.

At the time of intubation she was breathing fairly well, but still it was decidedly of a croupous nature.

Intubation was followed with perfect relief—tube removed on fourth day. Recovery rapid.

Case VI.—December 3, 1889. Age six years. Courtesy of Dr. P. N. Adams. Patient was in great distress from difficult respiration. Tube was introduced and respiration ceased. I quickly introduced the membrane forceps of Dr. Waxham and displaced the membrane that was evidently acting as a valve, and thereby saved the chagrin of death during operation. Tube was again introduced with relief. Patient died second day from extension of membrane. A second child in family died equally promptly two days later, without intubation.

Case VII.—December 20, 1889. Courtesy of Dr. H. S. Kimmel. Age one year. Nasal, pharyngeal and laryngeal diphtheria. Membranes were so extensive that we had no hope of saving life, and intubated merely for the purpose of affording comfort. Child lived two days with no return of difficult breathing. Death caused by diphtheritic toxæmia.

Case VIII.— December 23, 1889. Age seven years. Child had nasal, pharyngeal and laryngeal diphtheria in its worst form. Intubation relieved its difficult breathing, and continued to afford great comfort until time of death three days afterward, from diphtheritic toxæmia.

Case IX.—June 5, 1890. Courtesy of Dr. Shepherd. Dr. E. C. Davisson present. Age six years. Patient in very critical condition. Covered with perspiration, lips cyanosed. Found child held in the open air, and surrounded by the anxious family and neighbors, expecting every moment to be its last.

Complete relief of all distressing symptoms followed introduction of tube, as if by magic. Tube was coughed up on the 5th day, and was not re-introduced. Convalescence not rapid.

Case X.—July 14, 1890. Aged two and one-half years. Courtesy of Dr. J.S. Beck. Child was strong and operation done quite early. Case seemed to be a peculiarly promising one. Intubation yielded prompt relief. Breathing, however, in a few hours began to grow more difficult, and child died on the second day from extension of membranes.

Case XI.—September 24, 1890. Aged six years. Courtesy of Dr. Shepherd. Child had been sick with diphtheria for a week in Centreville, but had so far recovered that he came to Dayton prior to my first visit, two days. Examination revealed no membrane in pharynx. Dyspnœa quite marked. Tube introduced with complete relief. The act of swallowing quite well performed. Pulse rate soon ran up to 120; respiration to 48 to 50. Pneumonia was present in left lung. Tube removed on the morning of the fifth day. At noon the dyspnœa was so great that the tube had to be re-introduced, which was again removed four days later. Patient made a steady but very tedious convalescence.

Case XII.—October 7, 1890. Aged six years. Had been breathing in a croupy manner for about one day. Dr. Weaver was called in consultation, and we concluded not to intubate until the urgency of the case actually demanded it. After watching our patient two days, it became evident that something must be done. He was intubated with much relief. Coughing was very violent for several days. He swallowed liquid with great difficulty, but took more substantial food quite nicely. Owing to absence from city, tube allowed to remain in one week. Recovery was rapid.

Case XIII.—October 10, 1890. Age four years.

Courtesy of Dr. Shepherd. Patient brother of Case No. 10. Tube introduced with usual relief. For twenty-four hours everything seemed promising. Afterward respiration became more difficult and death resulted on second day from membranes descending.

Case XIV.—November 3, 1890. Age eight years. Courtesy of Dr. Bailey. Five in family sick with diphtheria. Patient moribund and unconscious, lips cyanosed. Tube introduced and respiration ceased. Tube was removed and artificial respiration instituted, and she soon was able to carry on respiration again, but in a very feeble way. The tube was found clogged with membrane. It was again introduced with magical results. Her whole being changed from that of death to life. Cyanosis disappeared. Pulse became recognizable, consciousness returned, and ability to talk to her parents once more took place. We almost begun to hope for a successful issue, but broncho-pneumonia ensued and she died on the third day.

Case XV.—November 6, 1890. Age twenty-three months. Courtesy of Dr. R. R. Petit. Little patient had had pharyngeal diphtheria some two weeks before I was called, and was considered cured; when unexpectedly, laryngeal diphtheria presented itself. When I was called patient was almost moribund. Lips greatly cyanosed. Intubation as usual produced instant relief of distressing symptoms. Pulse, respiration and fever were very high on account of broncho-pneumonia that was already under way. Nourishment was taken quite nicely. Feeling that presence of tube was longer unnecessary, it was removed on the fifth day. Patient took liquid nourishment quite well, but no better than when wearing tube. Patient lay in a very critical condition for nearly two months but finally became strong and well. At time of intubation I considered this a very unfavorable case.

Case XVI.—November 11, 1890. Age six years. Courtesy of Dr. Levi Spitler. Patient had been sick

several days and was in a very precarious condition. Tube afforded marked relief. He died November 15th after an heroic struggle for life. The cause of death seemed to be exhaustion from pneumonia and inability to take nourishment.

Case XVII.—November 19, 1890. Age two years. Courtesy of Dr. Corbin. Membrane in abundance was found in nose, pharynx and larynx. Tube was inserted merely for comfort. Patient lived one day, death caused by extension downward of membranes.

Case XVIII.—December 27, 1890. Age six years. Dr. Stein present. Patient had pharyngeal diphtheria, which had nearly disappeared when I was first called. Respiration quite labored and at times death seemed imminent. Intubation was not only advised, but urged with all the persuasive force at my command, but to no avail.

Several days passed and child continued to grow worse, until only a few hours more of life could be hoped for. By two hours argument, with the aid of the family minister I finally gained my point and introduced my tube. This afforded relief, induced sleep, did not seem to interfere materially with drinking of milk. Everything progressed satisfactorily and on the fifth day he coughed up tube, and convalescence was rapid and uninterrupted.

Case XIX.—January 15, 1891. Age two years. Courtesy of Dr. Crum. Membranes in nose, throat and larynx. Intubated merely for comfort. Died in eleven hours from diphtheritic toxæmia.

Case XX.—January 20, 1891. Age five years. Courtesy of Dr. Crum. Intubated easily, with perfect relief of difficult breathing. Case seemed a promising one. Died on second day from extension of membranes downward.

Case XXI.—February 14, 1891. Aged two years. Courtesy of Dr. J. H. Romspert. Nasal, pharyngeal and laryngeal diphtheria. Child almost moribund. Tube in-

serted only for comfort. Death occurred five hours afterward from diphtheritic toxæmia.

Case XXII.—February 24, 1891. Age six years. Courtesy of Dr. Crum. Patient intubated with usual relief. In twenty-four hours respiration had become exceedingly difficult and death imminent from extension of membranes downward. Parents, through the advice of a German practitioner, demanded tracheotomy. This I reluctantly consented to do on account of the exhausted condition of the patient.

Almost as soon as the chloroform had begun to be administered child ceased breathing. Tracheotomy was hurriedly performed and respiration was again established. The hemorrhage, while quite considerable, was not excessive; but in his exhausted state this and the shock was too great, and patient gradually sank and died in about an hour.

Case XXIII.—March 1, 1891. Age eight years. This case came into my hands after being considered hopeless, and abandoned by another physician. Intubation afforded complete relief of respiratory trouble. Pneumonia was already present Liquid nourishment taken very badly and solid food refused. Tube was removed on fourth day—patient lingered thirty-six hours and died from exhaustion. With better home care and attention, this patient ought to have recovered.

Case XXIV. — March 8, 1891. Age six years. Courtesy of Dr. Stein. Patient had had an attack of pharyngeal diphtheria, but membranes had disappeared and family were congratulating themselves on their good fortune, when difficult breathing gave proof of the existence of membrane in larynx. Tube was inserted that gave absolute relief. Tube was ejected on fourth day and rapid recovery followed.

Case XXV.—March 15, 1891. Age two years. Courtesy of Dr. McIlheny. Introduction of tube gave im-

mediate relief. In twelve hours I was hurriedly summoned on account of return of difficult breathing. I found tube absent and introduced another with complete relief again. At end of second day I was again summoned on account of the presence of same symptoms. This time, however, the tube was in place, and trouble caused by extension downward of membrane. Child died on the third day. The lost tube was discharged from rectum 48 hours after being swallowed.

Case XXVI.—April 8, 1891. Age nine years. Membranes present in nose, pharynx and larynx. Relief by intubation immediate, but continued only a short time. Death occurred in seven hours from occlusion of trachea by membrane.

Case XXVII.—April 16, 1891. Age four years. Courtesy of Dr. Kimmel. Nasal, pharyngeal and laryngeal diphtheria present. Intubated only for comfort. Child died on the third day from diphtheritic toxæmia.

Case XXVIII—April 16, 1891. Age four years. My own patient. Diphtheritic membrane in throat, nose and larynx. No hope of recovery. Intubation demanded by parents, who had seen relief afforded in another case. No return of dyspnœa occurred after intubation. Death ensued on fifth day from diphtheritic toxæmia.

You have noticed undoubtedly in my report of cases that I have not advised against an operation even when I was positive of a fatal issue. I have not watched my statistical record with that care observed by some who are seeking glory rather than relief of suffering. Some refuse to intubate in unpromising cases for fear of bringing the operation into disrepute, but it is my belief that if the family are informed in advance that you only expect to produce comfort that the operation will not suffer. Some of my families where fatal results followed are the loudest in their praises of intubation.

Intubating every case that presented itself to me,

twenty in the hope of saving life and eight only for comfort, I have 28.5 per cent. of recoveries. If I had refused to operate in the eight cases that I considered absolutely hopeless, as would have been the case had tracheotomy been the operation under consideration, my per cent. of recoveries would have been forty.

Waxham has collected 1072 cases of intubation, with 287 recoveries, or 26.7 per cent. Dr. Brown, of New York, reports 2368 cases, with 647 recoveries, or 27 3-10 per cent. Dr. Stern, of Philadelphia, reports a large number of published cases of tracheotomy for pseudo-membranous laryngitis, with 26.4 per cent. of recoveries. Considering the fact that published cases are generally the reports of the more fortunate operators, this per cent. probably far exceeds that which we would get from private inquiry. In Dayton, so far as I can learn, 12 tracheotomies have been performed for membranous croup, with 12 deaths. That this result is not due to inferior surgery is shown by the fact that eight of these operations have been done by one man, who has also done eight tracheotomies for removal of foreign bodies with eight recoveries.

In the face of such reports as these, especially when we consider that it is the exception when tracheotomy will be allowed, and often inadmissible from the exhausted condition of patient when first seen, it seems strange that there are those who still condemn intubation, and prefer to sit idly by and witness the poor sufferer die from strangulation. They seem to argue in a manner quite similar to a learned president of Yale College in years gone by who publicly denounced vaccination, claiming that it was God's design to destroy a certain portion of mankind by small-pox and to thwart that purpose by the trick of vaccination was evident proof that it was the invention of the devil.

While I have confined my paper to intubation for laryngeal stenosis caused by membranous exudations, it has a broader range of application. It has been employed suc-

cessfully in acute œdema of the glottis, as well as in stenosis due to granulations, syphilis and papilloma.

While we laud intubation as the greatest step forward ever made in the treatment of pseudo-membranous laryngitis, and are pleased to witness conversions of its enemies, still we are not satisfied with its results and never can be until more than 30 per cent. of recoveries can be obtained. We hope and trust that ere long some new mechanical contrivance or medicinal means will be found that will make us not merely helpers but actual masters of this dread disease.

DISCUSSION.

Dr. Thos. Hubbard:

I think the doctor has made a very strong argument in favor of intubation, but I don't think he has put it quite strong enough in his presentation of statistics. He has included in those statistics operations which were performed without the hope of success; that is not fair. The statistics of Dr. Waxham, which I have by letter from him, illustrate the progress of this life-saving operation. In his first 100 cases, he had 27 per cent. of recoveries; second 100, about 34 per cent.; third 100, 42 per cent.; and the last 42 gave 38 per cent. That is certainly a remarkable result. I want to compliment certain features of his operation, from my personal observation of his methods. It takes courage to introduce and remove a tube, in the face of the remonstrances and even threatened restraint on the part of the family, and often without any support from the consulting physician. In a case where the tube has been introduced and the membrane has evidently gone below the tube, instead of waiting for that membrane to close the lower orifice of the tube, or have the tube rejected, he withdraws the tube, and again introduces it, attempting in that way to loosen the membrane. I have never seen him use the forceps, but instead of that he introduces and withdraws the tube many times, in hope of dislodging and bringing out a piece of the membrane. In these cases he also uses a cylindrical tube. It is of such a shape that it can be easily coughed out, and there is less danger of a

stoppage of the tube. In feeding, the patient's head is drawn over the edge of the bed. Of course that is a very difficult proceeding at first, but the child soon yields, and readily takes nourishment in teaspoonful allowances. I have seen cases where there was absolutely no coughing following the taking of liquid nourishment after this manner.

Dr. John North:

I was very much pleased with the paper, and with the results obtained by the author. I do not think the doctor's statistics do justice to himself; eight of the cases reported were beyond any hope of recovery before seen by the doctor. If he had left out these cases, he would have had a much better report to make. He simply operated on those cases to relieve the patients so that they might die easy. I think that one of the greatest blessings of the operation is that we can relieve the patient, and prevent him from dying from strangulation. I believe in time it will take the place of tracheotomy for laryngeal stenosis in a large majority of cases.

PAPILLOMA OF LARYNX; WITH CASE REPORTS.

BY A. B. THRASHER, M. D., CINCINNATI.

December 2, 1890, Mrs. S., æt. 64, consulted me for loss of voice. She had been troubled with hoarseness for two or three years, and had not been able to speak above a hoarse whisper for over two months. Her parents had died from unknown causes, between 80 and 90 years of age. There was no history of hereditary or acquired constitutional disease, and she was the mother of a large family of healthy children. There was no cough except occasional attempts to clear the throat.

Examination revealed interstitial hypertrophy of both tonsils. The epiglottis, ary-epiglottic folds and ventricular bands were slightly congested. The posterior third of the right vocal band was covered by a red granular tumor, springing from the edge and apparently both surfaces of the cord, and projecting, in moderate breathing, across the chink of the glottis and impinging on the opposite vocal band. During attempted phonation the vocal bands could be brought in contact only for a short space at the anterior commissure, while the neoplasm was crowded mostly below the orifice of the glottis. My diagnosis was papilloma of larynx. After anæsthetizing the parts with cocaine I removed the growth *en masse* with my antero posterior cutting forceps. The specimen was submitted to Drs. Oliver, Freeman, Holt and Cameron, microscopists to the Cincinnati Hospital. They reported that the neoplasm was composed of epithelial cells and

connective tissue stroma, and was of uncertain character but probably an epithelioma.

The wound healed kindly, and in six weeks the voice was nearly normal and there was a red indentation on the vocal band at the former seat of the tumor. Two weeks ago, six months after the operation, the entire edge of the band including the scar tissue was white and healthy in appearance.

Case II.—Mr. E., æt. 36, Lafayette, Ind., consulted me last April for a husky voice, at times very much worse than others, dating back three years or more. There was irregular cough, the exacerbations of coughing accompanied by almost complete aphonia.

No history of tubercular or syphilitic disease, either family or personal. Examination revealed a smooth reddish growth of the size of a cherry occupying the interarytenoid space and springing from the posterior wall of the larynx. There was no objective evidence of disease in lungs or other organs. There was some laryngeal hyperæmia but not a severe laryngitis. A portion of the tumor was submitted to the microscopists, who reported it a simple papilloma. The growth was removed, the base cauterized with galvano cautery and the voice and cough soon improved.

These simple cases will serve as an illustration of the few remarks I desire to make on the subject of benign neoplasm of the larynx. In Case I. the age of the patient would raise the question of differential diagnosis between warty, syphilitic, tubercular, and malignant growths.

My first diagnosis of papilloma was made from the subjective absence of pain and the objective presence of hyperæmia without violent inflammation. In syphilis there probably would have been some other manifestation of the disease and a higher degree of congestion. In tuberculosis, along with constitutional symptoms or manifestations in other organs, notably the lungs, there would have been more tumefaction and less hyperæmia. Espe-

cially would there have been tumefaction, translucent in appearance, more marked over the posterior extremities of the ary-epiglottic folds and the under surface of the epiglottis. There is not always in tubercular laryngitis the anæmia of the larynx which we are taught is pathognomonic of the disease, especially when there is active inflammation or ulceration; but there is general anæmia well observed in the pharynx, or elsewhere, which was absent in this case.

In Case II. the neoplasm was situated in the usual place for tubercular nodules The age of the patient, the cough, the general appearance, all might indicate a tubercular affection. Yet there was no tumefaction over the posterior extremities of the aryteno epiglottidean folds, no ulcerations, no anæmic appearance of the laryngopharyngeal mucosa, nor any physical indications of disease of the lungs.

The inflammatory zone surrounding a malignant growth is usually well marked, and the violence of the inflammation is out of all proportion to the size and appearance of the central lesion. Pain is usually a well marked and prominent feature of malignant disease; not always felt in the seat of the neoplasm, but not infrequently radiating to the ear, or cervical glands.

The first case was referred to me as one of malignant disease of the larynx, and the microscopical examination of the neoplasm pointed in the same way. Yet at the first laryngoscopic examination I felt confident that I had to deal with a simple papilloma and the subsequent history has justified the diagnosis. The second case was sent to me with the diagnosis of tubercular laryngitis, and while the pathologists say the growth was a papilloma and the appearance of the tumor in the laryngoscopic mirror was the same, yet it is too soon to say that it was not caused by a tubercular diathesis. Here, as elsewhere microscopical examination of the neoplasm is not always, or, I might say, ever, a sufficient basis for diagnosis. The

clinical history of the disease and the subjective and objective symptoms play a most important part. Even when all precautions have been taken we are frequently enough mistaken, and only corrected by the subsequent history of the case.

Cough, even when chronic, does not always mean a pulmonary affection, and aphonia may not mean a disease of the larynx. The careful interrogation of every organ likely to be affected will sometimes throw a curious light on what at first seemed a simple condition, or may render very simple what was before very obscure.

TONSILLOTOMY AND ITS AFTER-TREATMENT.

BY T. V. FITZPATRICK, M. D., CINCINNATI.

There is an abundance of clinical proof of the relation hypertrophied tonsils sustain to the production of such phases of ill-health as middle-ear disease, cough, impaired phonation, imperfect respiration, impaired nutrition and retarded mental growth. To attempt to give the clinical histories of cases that have been under my observation, or a résumé of the literature, in support of the above statement, would appear platitudinous. I cannot agree with the statement that the ill-health following hypertrophied tonsils is mainly due to the inspired air passing over a diseased surface. We often have instances of the inspired air passing over large surfaces of diseased mucous membrane without the results that usually attend enlarged tonsils. The disturbances of the general health and retarded mental growth are directly the result of the impeded respiration, and consequently the imperfect aeration of the blood. The peculiar shape of the thorax is largely due to the increased action of the accessory muscles required to overcome the resistance of the diminished lumen of the upper portion of the air-tract. The enlarged tonsil is made up largely of fibrous tissue, which of itself is not harmful, but when its presence interferes with the proper performance of the function of breathing then it becomes pernicious. In many instances the lymphatics have been found to be greatly enlarged. If the normal resistance of the tonsil to extraneous matter should be diminished, then these enlarged and active lymphatics may become a channel through which infec-

tious matter could find easy access to the general system. It may be due to this fact that the acute infectious diseases are manifested first in the throat.

The relief of these pernicious conditions is sought for in the removal of the tonsil, and I believe that the prevention of such conditions should be sought for in the same manner That the removal of the tonsils will accomplish the purpose for which it is done cannot be for one moment doubted. There is, however, a barrier at present in the way of the universal practice of tonsillotomy; that is the idea that is quite prevalent that the amputation of the tonsils is sometimes attended with fatal results, and very frequently with alarming hemorrhage. I have examined as thoroughly as possible all the literature that I could command, and could find but one (Ricardeau, Paris, 1886) case of fatal hemorrhage following tonsillotomy for simple (non-malignant) hypertrophy of the tonsils. The report of this case is wanting in details; the only statement noteworthy in the report is that the young man was twenty-four or twenty-five years old. No mention is made of the means used to arrest the hemorrhage, or whether the hemorrhage was primary or secondary. Dr. D. Bryson Delavan makes the statement that there is not a single well-authenticated case on record of fatal hemorrhage following tonsillotomy with the tonsillotome for hypertrophy. Not a single author, though all admit the great infrequency, gives the report of a single case of fatal termination. With the possible exception of the above somewhat obscure case, I do not believe there has been, at least for the last twenty-five years, a single fatal case. Therefore, I think that the possibility of a fatal issue is reduced to such a minimum as to play no part in the question of tonsillotomy. That there is possibility of a hemorrhage of an alarming character, I am frank to admit, but, fortunately, it occurs with great infrequency. If there was danger of death or of frequent alarming hemorrhages, it

would indeed be the highest degree of rashness, if not of criminal carelessness, to subject a patient to an operation more disastrous than the disease for which the operation is done.

In examining the literature of the last twenty five years, I find the report of forty two cases of hemorrhage following the removal of the tonsils. Only one of the whole number was under eighteen years of age. This is a very significant fact. Why, then, does age favor hemorrhage? Does it play such an important part in other operations? The only explanation of its relation to tonsillotomy is that in those who have had hypertrophy of the tonsil for a long time there is developed a hard, fibrous tissue running through the tonsil; the blood-vessels thus surrounded are prevented from contracting and retracting sufficiently to arrest the bleeding. Dr. Morell Mackenzie says he has "only known of one case, where hemorrhage actually seemed to endanger life, out of one thousand reported cases." Dr. George M. Lefferts has had but two cases of severe hemorrhage in five hundred cases. In one hundred tonsillotomies I have not seen even a serious hemorrhage.

One is safe in the belief that tonsillotomy has been performed many thousand times in the last twenty-five years, with but forty-two reported alarmingly severe hemorrhages. These statistics are of much value in pointing out the class of cases that are likely to prove bleeders after tonsillotomy, viz., adults. As shown by these statistics, only one case of alarming hemorrhage occurred under eighteen years of age, in many thousand tonsillotomies; therefore the conclusion is forced upon us that since enlarged tonsils in the majority of cases produce, sooner or later, pernicious results, they should be removed before the pathological changes have reached that degree of firmness of structure as to make an alarming hemorrhage possible.

If there is a history of a disposition to bleed alarmingly

from slight causes, then other measures than the tonsillotome should be resorted to, such as the galvano-cautery knife. Not only would the early removal of the tonsils prevent the pernicious inroads on the general health, which can be confidently expected in the majority of cases, but it would also reduce the probability of an alarming hemorrhage.

A recent statement by high authority in gynecology is that enormous ovarian tumors were getting to be a matter of history, because the family physician was skilled in the early detection of such growths. I feel confident that when the evil results of hypertrophied tonsils are fully realized, and the assurance of the almost absolute safety of tonsillotomy is established in the mind of the family physician, then hypertrophied tonsils in adults will also be a matter of history.

The technique of tonsillotomy is not difficult to acquire, yet several of the reported cases of alarming hemorrhage were due to bungling operating, dividing the pillars of the fauces, puncturing the internal carotid artery, etc. If the precaution were always taken to break up adhesions of the pillars to the tonsil, and using a tonsillotome instead of a bistoury, such sources of danger could be easily avoided. Clinical experience has demonstrated that if only that portion of the tonsil which projects beyond the pillars is removed, atrophy of the stump will soon take place, accomplishing just as beneficent results as if the entire tonsil had been removed. Then the danger of wounding the internal carotid, which I doubt ever happened with the skillful use of the tonsillotome, or the danger of dividing the tonsillar arteries at a point where they are sufficiently large to do much bleeding, would be obviated.

Tonsillar hemorrhage from a clinical standpoint may be divided into two classes—primary, or that which immediately follows the tonsillotomy, and secondary, or that which comes on after sufficient time has elapsed for

the formation of a clot. I regret that the majority of the cases reported have made no mention of how soon after a tonsillotomy the hemorrhage began.

There are three classes of causes, either of which may be sufficient to induce secondary hemorrhage: (1) Mechanical displacement of the clot by coughing, clearing the throat, or by swallowing solid food, or by the combined action of all three; (2) decomposition of the clot; (3) sloughing of an artery when a part of the coats have been divided in the operation.

The stump is about as much exposed to septic influences as it would be if it were upon the surface of the body, and that, too, without any possibility of applying antiseptic dressing.

I have nothing new to add to the means used to arrest tonsillar hemorrhage. The one remedy, however, most favored is one part of tannic acid to two parts of gallic acid, of the consistency of syrup. The patient is allowed to swallow it. I believe this is a better way than to apply with a brush. I believe gargling or swallowing water as hot as can be borne would have the two-fold effect of a styptic and antiseptic. Transfixion of the stump and the application of a ligature behind the transfixion-needle should always be resorted to before ligation of the common carotid.

It is very important that the patient be prepared for the operation. The thorough cleansing of nasal cavities, throat and mouth; the bowels should be moved previously, and every means used to prevent excitement. Mental excitement superinduces rapid action of the heart, with increased arterial tension, which very considerably favor a hemorrhage.

As soon as the tonsils are removed an antiseptic application should be made to the stump. I use a 1 to 1,000 bichloride solution at the time of the operation, and a 1 to 4,000 solution as a gargle, to be used frequently for a few days after the operation. This method of after-treat-

ment will give the patient greater security against the decomposition of the clot.

The patient should be instructed not to eat solid food or make any violent efforts at clearing the throat or coughing until the healing process is quite complete.

We are justified in drawing the following conclusions:

1. That the only reported case of fatal hemorrhage needs further corroboration before it should influence the usefulness of tonsillotomy.

2. That age is of all other conditions the most prolific cause of hemorrhage.

3. That the early removal—that is, before the tonsillar tissue has become hard and fibrous—will reduce the probability of an alarming hemorrhage to the ratio of one to all the reported cases of tonsillotomy.

4. That if the operation is skillfully performed with a tonsillotome, and the throat subsequently properly cared for, the probabilities of even a slight hemorrhage are very small.

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DISCUSSION.

Dr. A. DeVilbiss:

I arise to commend the paper in every respect. That which he says about the tonsillotome is correct, and I

commend it. I want to speak about the use of the cautery knife. Dr. Wright, of Franklin, has got a cautery tonsillotome, and the danger of cautery to the pillar is relieved. There is no danger of opening into the artery, and no harm is done. It is simply foolish to let a child go, when the operation in skilled hands is so easily done.

Dr. J. McCurdy:

For a good many years I have let my tonsillotome rest on the shelf. I have taken the tonsils out, and pretty near the whole throat. My plan now is to either use carbolic acid or tincture of iron, full strength, and make the application, at the same time giving iodide of potassium, tincture of iron and arsenic. In my hands I have had no failure. It is very easy, and I admit that removing the upper half of the tonsil can be done, except you have an ignorant parent and an unruly child, and then it takes a mighty smart man to get any part of the tonsil out.

Dr. Thos. Hubbard:

As the essayist has well stated, hemorrhage following tonsillotomy is generally due to the dense fibrous tissue which surrounds the artery, thereby preventing the natural formation of the clot, and especially where the incision is made with a sharp knife. I believe that many of these dense fibrous tonsils could be amputated successfully with a dull instrument, thereby lacerating the coat of the artery sufficient to aid in natural clot formation. I recently had a case where the operation was performed with sharp scissors. It was a tonsillar polypus, small pedicle, and I unhesitatingly snipped it off. After four hours there was violent hemorrhage. Then the hemorrhage ceased for four days, and then occurred for four hours. On both these occasions the young man bled to the verge of complete syncope. In this case I demonstrated microscopically, that from the size of the blood vessel and the dense fibrous tissue around it, it would be difficult for a clot to become firmly anchored. Very many cases of recurrent suppurative tonsillitis are caused by the adhesions between the anterior or posterior pillars of the fauces and the tonsil. When these are broken up,

the tonsil is liberated, and resumes its normal position. Otherwise a simple pharyngitis runs into a peritonsillar inflammation. Instead of ligating the tonsil after transfixing, transfixion can be done with a curved needle in firm handle, and by a simple rotatory motion the tonsil can be so twisted upon its axis that hemorrhage will cease. The patient can hold the handle of the needle between the teeth, and thereby you have a very simple and sufficient method of preventing much hemorrhage.

In all cases where I anticipate hemorrhage I now use the galvano cautery knife to cut half way through the tonsil, leaving a groove above and below so that a cold wire snare can be slipped over the now pedunculated mass, and complete the amputation with no risk whatever.

Dr. J. North:

We speak of hypertrophy of the tonsil, which I think is a misnomer. We have a hyperplasia, or an excessive enlargement of the tonsil, not a true hypertrophy. I believe in children, before the so-called tonsils enlarge, there is a physiological condition in which we do not have any tonsil. It is simply a folding in of the mucous membrane, giving us the form of racemose glands—that is, there are no glands there, in the sense we ordinarily understand the term gland. When we have any enlargement, it is not an hypertrophy. In hypertrophy we simply have an enlargement of the normal tissue. Some persons do not have any tonsils. We feel in between the pillars; there is no tonsil there; where there is an enlargement it is hyperplasia from some condition, not hypertrophy. I am not going into what causes this, but we do have this material collected there, and it should be removed. It interferes with the voice; it interferes with singers very much. There is a space between the pillars that acts as a resonator to the voice. The fibers of the pillars and space between them have something to do with the formation of sounds, particularly in singing. I usually have advocated removing these so-called hypertrophied tonsils. I have tried to cauterize them, but have never had very good results, because but a portion of the tissue is removed thereby, and some of the racemose glands are allowed to remain there, and they will fill up with secretions, and act as a source of irritation, and that is

very apt to be followed up with suppurative tonsillitis. I don't believe in removing a portion of the tonsil. This tumor should be removed, and all of it. If there is a portion of it removed, that which remains contracts to a certain extent, and leaves the surface which remains free to take on an inflammatory condition when exposed to cold, etc. So I believe that we should remove the tonsil entirely. Care should be taken in these cases. I have operated upon a great many cases. I have operated on three children under three years of age within the last ten days, and had no trouble whatever with hemorrhage, and removed the tonsil entirely. I believe this is the correct practice. We do not have as much hemorrhage as we do to cut off a portion and leave the mouth of the vessels wide open.

Dr. Williams:

I have been very much interested in the paper. I have been taught that there is comparatively no bleeding in excision of the tonsil; but I have had occasional bleeding, and that has convinced me that there is some bleeding to be apprehended. I had it in one patient about 20; the other was 15. A lady came to me, who weighed about 190 pounds. I put cocaine on the tonsil at the time, with very little pain, but there was terrible hemorrhage. It was for two hours in my office that she bled off and on. She then went to her home, about a mile away. About an hour afterwards they sent for me to come. She was lying on a bed, almost pulseless, of a clay color. She had been bleeding profusely. I stayed a couple of hours with her. I put persulphate of iron on the tonsil, and stopped the bleeding. She lay prostrate for almost a week. That evening and during the night she had two large stools, and evacuated a large quantity of clotted blood. The other was a girl about 14 years old. The hemorrhage was pretty much the same color, but not quite so much. Sajous speaks of two cases. In his first work he says there is no bleeding; in his last work he speaks of two cases that had been reported to him, of deaths. He doesn't say who reports, but he says he had had cases reported lately, in which there had been almost fatal hemorrhage. Since those cases of mine, I have taken the precaution to inject cocaine in the tonsil. I have never had any bleeding since. I would like to get some information as to the treatment of the stump. I use nothing but cocaine.

Dr. S. S. Thorn:

I rise especially to enter a protest against the use of the instrument Dr. DeVilbiss spoke of, the electric apparatus, cutting off so as to prevent the flow of blood.

Dr. Devilbiss:

That is in the case of a fibroid tonsil.

Dr. Thorn:

That is the very kind of a tonsil I object to. I don't believe in ligation. I have used the ligature, and I don't want any of it. Especially I want to protest against the bringing to life again of that most diabolical of all of them, persulphate of iron. I had hoped that the last man who used persulphate of iron had been gathered to his fathers.

Dr. A. B. Thrasher:

I want to interpose a slight objection to one point of the paper, and that is as to the use of an antiseptic gargle after tonsillotomy. By the action of the faucial muscles in gargling there is a liability of disturbing the blood clot and keeping up the hemorrhage. By means of a coagulating antiseptic placed on the cut surface by means of a brush the hemorrhage can usually be controlled. It is impossible to bring about entire antisepsis, consequently it is better to leave the clot alone.

The tonsil is just as different in size, normally, as noses are different in size, consequently, not all large tonsils should be removed. It is not necessary to cut out a tonsil because it is large, unless it is by reason of some pathological condition. It is a very proper thing indeed, when there is an obstruction to breathing, or when the tonsil is hypertrophied. That very frequently is the cause of difficulty in breathing, and then it must be removed. There is no doubt that the tonsil sometimes shrinks after the use of iodine, but not because of the use of it, as frequently a tonsil will shrink anyhow. A tonsil can be burned out by acids, but it is very much more troublesome than removal by the knife. It is not so easy on the patient, and it is not so certain a process, because you can't tell how deep the carbolic acid is going to go. I believe there hasn't been a single authentic report of

death from the use of the tonsillotome. I have heard of several cases in which the internal carotid artery has been ligated, but of course, gentlemen who have studied anatomy, know that this could be of little or no use. But there has been no fatal case--there has been no case where the bleeding could not have been stopped by pressure. You can control by pressure until the difficulty is over.

Dr. A. B. Walker :

I consider the removal of a large fibrous tonsil a very dangerous operation. I had a case, that of a gentleman aged 38 years, who had very large tonsils, that I removed with a tonsillotome, and an alarming hemorrhage followed.

Dr. Thrasher :

Whose tonsillotome did you use?

Dr. Walker :

I used Mathieu's tonsillotome. The hemorrhage ceased soon after the tonsils were removed, but in a few hours afterward he bled profusely and grew very weak. I succeeded, however, in checking it by applying Monsel's solution pure, which caused a perfect clot to form on the remaining part of tonsil. This clot remained for several days, after which I had no further trouble.

I at first thought the hemorrhage was due to the use of cocaine, as I had the parts thoroughly anæsthetized, but after examination of the tonsils concluded that it was due to their being of a hard fibrous nature, which prevented the blood vessels from contracting in the hard tissue that was left. Since this experie ce, have approached all these cases with caution and care. I never have had trouble from hemorrhage before nor since this case, but I do feel that the removal of fibroid tonsils by cutting with the knife or tonsillotome is attended with danger from hemorrhage.

Dr. T. V. Fitzpatrick :

A previous speaker referred to the use of tr. of iodine and carbolic acid in the treatment of hypertrophied tonsils. Such a method of treating hypertrophied tissue is

slow and often very unsatisfactory, whilst on the other hand the use of the tonsillotome rapidly removes the diseased tissue, is always satisfactory. I cannot agree with the statement of a speaker that the difficult breathing is not due entirely to the enlarged tonsils. Whilst it is true that there is associated with many cases of tonsillar hypertrophy adenoid vegetations of the naso-pharynx and nasal hypertrophies, yet it has been my experience in many cases that the removal of the tonsils thoroughly relieved the impaired respiration. From a clinical standpoint it does not make much difference whether the enlarged tonsil is a true hypertrophy or a true hyperplasia; it is a foreign body both on account of its size and the relation it sustains to the lymphatic system, and should therefore be removed. The fact that all the cases referred to by previous speakers, where tonsillotomy was followed by severe hemorrhage were adults, corroborates my statement that age is the most important factor in considering the advisability of removing the tonsil. It can not be too strongly insisted upon that children having enlarged tonsils attended with pernicious results, however slight, should have the tonsils removed early.

HOME *VERSUS* HOSPITAL TREATMENT OF ACUTE INSANITY.

BY A. B. RICHARDSON, M. D., CINCINNATI.

It may be objected to the subject of this paper, that the time of the society is needlessly consumed in the discussion of a subject around which there is so much mystery, and for the treatment of which it is impossible to formulate rules of guidance which will be available for the ordinary physician in general practice. Though we concede that mental disorder is a disease, and as such within the province of medical practice, and though we insist that insane hospitals shall be placed in the hands of medical men, when we ourselves come into contact with the disease, we can scarcely control our instinctive impulses to view it in the light of an intangible something without a basis in tissue derangement, and without anything which will give reliable guide to its prognosis. The past year has been a revelation to me in some respects, and I have been so struck with the uncertainty and indefiniteness in the minds of even the most expert practitioners in other lines of work, when in the presence of mental disorders, that I have felt that something might be done to clear up some few points at least in this vaguely comprehended field of pathology.

First then, I seriously object to any view of insanity which does not recognize not only that it is a disease based on physical disorder, but also that in its consideration the exact physical changes shall be kept prominently before the mind of the examiner in every view of the

case. The mental manifestations are simply the symptoms of the disorder of function, and underlying them we must seek for the indications of the actual disease.

There has been great haziness and a very large unknown quantity connected with the pathology of acute insanity, and we must not judge too harshly those who have been the investigators in this field because greater progress has not been made. The investigation of its pathology is surrounded with unusual difficulties. The tissues which are the seat of the pathological changes are exceedingly complex and delicate in structural arrangement. The preparation necessary for their successful examination, of itself, often destroys the evidence of the pathological change, and while we are making great progress in connecting function with tissue there is yet much that is unsettled. As long as physiology has not blazoned the way, pathology must remain handicapped and work blindfold or in uncertain light. During the past few years, however, by advances in the field of brain physiology and by improved methods of preparation of brain tissue, much has been done to elucidate the pathological changes in acute insanity.

It may seem strange to you, gentlemen, that I dwell so long on this question of pathology in the discussion of the subject of this paper, but we can make no progress in the study of the treatment of a disease until we have some just conception of its nature. I have been so impressed with the statement of some of my friends in the practice of medicine in investigating cases of mental disorder that they could find absolutely nothing to account for the symptoms, and after autopsies in such cases they could see absolutely nothing to account for the demise of the patient, that I am constrained to believe that a few moments' consideration of this subject will not be time lost. Of course they could not, because they look for something which does

not exist. The pathology of acute insanity does not disclose itself to macroscopic view, except in rare instances, and the microscope even will only reveal these causes when the investigator has been trained by long experience in the preparation of these tissues and knows what he can expect to find. A knowledge of the minute anatomy of the cerebral cortex is the first requisite. Without this a just conception of the pathology of acute insanity is impossible. Then the relations of physiological activity to this must be carefully investigated. Having determined these as far as they can be settled, we must then make a study of the symptomatology in its relation to this anatomy and physiology, and connect this as best we may with the various pathological changes found. Only then are we competent to determine what the course of an attack will probably be, and what form of treatment will be best adapted to it. In this case much uncertainty must even then remain, but this will not excuse us from doing our duty as far as it is known.

Let us sketch briefly an outline of the pathology of a case of acute insanity. The most typical of these are seen in that form which begins in the cerebral cortex itself and from its over-excitation.

With normal cerebral activity there is connected physiological hyperæmia, and this hyperæmia is controlled by the vaso-motor and vaso dilator system of nerves through the molecular changes of the cerebral cells connected with their functional activity. This hyperæmia, when prolonged or excessive, disturbs this vaso-motor control; the products of the excessive metabolism in the cells also impair their functional activity and tissue changes begin to manifest themselves. The blood-vessels remain dilated, they dilate irregularly, their tissues become changed in structure; leucocytes pass through them and block up the peri-vascular lymph spaces which surround every vessel of the cerebral cortex; blood

pigment and granular matter also fill up these spaces. The brain cells, which are directly connected with these lymph spaces by channels and themselves surrounded by a lymphatic envelope, lie bathed in the products of their excessive metabolism, and soon begin to show the effect of this poisonous fluid and their deranged nutrition. They become granular, lose their normal outline, are swollen, their nuclei are displaced, lose their distinct outline, contain an excess of pigment, and fat globules begin to show themselves in both cell and nucleus. From this on the changes are more plainly evident. With the progress of the disease the cell prolongations become rounded off, the association fibres are diminished in number, the entire cortex is atrophied, the small vessels become often entirely occluded, and miliary aneurisms cut off the nutritive supply from still other portions. Is it any wonder that we have perverted function in such cases, and is there not sufficient evidence to explain the mental symptoms, the enfeeblement and perversion, long before the eye can detect these changes? Then, too, the centres of the vital functions, the respiratory centres and the centres of cardiac innervation, depend upon the anatomical and physiological perfection of just such microscopic cellular elements, and it is not surprising that we have death itself when the eye can detect no changes which would account for such grave results.

Now, as citizens we are interested in the prevention and proper treatment of insanity, because we know not in what manner it may touch our interests or invade the circle of our friends. With the tide of civilization, in its restless, impetuous and resistless course, come the wrecks of these weakened and storm-beleaguered vessels. The higher and the more resistless the tidal flow, the more frequently do these wrecks appear, and the greater the responsibility of those whose duties are to protect them from utter destruction and to guide them safely through the breakers.

In the care of the insane a number of considerations arise. The first must always be the restoration of the normal brain functions of the individual. A second is to secure at the same time his protection, as well as that of those with whom he is brought into contact. A third is to secure these ends with as little jeopardy as possible to the patient's social and financial interests, and a fourth, and more subsidiary, to accomplish the patient's restoration as economically as possible and with as little financial loss and mortification to friends and relatives as is practicable under the circumstances. The moral and social aspects of the treatment are unfortunately, perhaps, among the most essential to be considered. I say unfortunately, because as physicians we should strive to view insanity solely as an index of brain disease, and treat it with reference to this alone. It is true that even as physicians we find it difficult, at times, to connect disorders of the personality and perversions of the conscious ego with structural derangements of the cerebral cortex, but the connection exists and the structural change should always be kept in mind. We should have some intelligent mental picture of what it is, and what its probable course will be.

In insanity the mental symptom of most importance, one which oftenest demands attention and most frequently determines many elements of the treatment, is the disorder of that function which controls the individual's conduct in health—his volition. His volitional control is perverted and defective, and, as a consequence, the regulation of conduct becomes one of the prominent considerations in the management of the case. This greatly complicates the treatment, and often interferes with that adaptation of it which would be most advantageous to the diseased condition of the brain underlying the perverted function. In no other disease is there so strongly-marked individual peculiarities. Each case is a law unto itself, and must be studied in the light of its peculiar manifestations.

As to forms of treatment, that is, of course, best which most promptly and most effectually restores the individual to his normal state. Owing to the disorder of volition, which is an accompaniment of every case, the plan of treatment will be the first point to be determined. Three plans are usually at command, sometimes only two. They are: home, the residence of friends, or a specially constructed institution. Which shall it be? In the large majority of cases, the particular symptoms in the case, the want of proper provision at home, or the presence of some jeopardizing influence in it, will require removal to an institution especially adapted to insure the protection of the patient. The question of safety becomes paramount, and decides in the favor of hospital treatment. In other instances, however, this consideration is not so imperative. The patient does not manifest any tendencies endangering either his own safety or that of his friends, and yet the home surroundings are such that attempt at treatment there would be decidedly prejudicial to his interests. Then removal to the home of a friend may become advisable, providing it admits of all the other necessary requirements. In still other cases, and of the total number of cases of acute insanity occurring in a community they form by no means an insignificant percentage, the sole question to be decided is where can the medical treatment of the case be best conducted, and what place offers the best chance for recovery, the safety of the patient or his friends not being an element in the case at all. Then it is that we are called upon to decide, from the indications present, what the course of the disease will be, and how long it will probably continue.

Are there any indications which will enable us to determine this, and can we form any reliable conclusions as to duration and probable outcome from the symptomatology? I believe there are many such, and that a careful study of each case, when we have previously pre-

pared ourselves for an intelligent interpretation of the symptoms, will enable us to give reliable advice in by far the larger number of such cases.

In the first place, in most instances, insanity, as is true of other diseases, runs a definite course. It will require a certain time for its removal, and though proper treatment may control this within certain limits, beyond this we have no control of it. It will be a matter of some weeks, at least. In the second instance, all cases in which depression is a symptom, should be looked upon with suspicion. These cases usually run a more protracted course, and there is always the possible suggestion of suicidal impulse which should cause a careful guard to be placed upon the patient's movements at all times. Every case of melancholia is a possible suicide, and should receive the protection of such cases. If this cannot be securely afforded at home, than recourse should be had to a hospital.

Again, if the excitement or violence of the patient is such as to require forcible restraint, and this cannot be readily overcome by conservative medication, home treatment is not advisable. Great excitement, frenzied delirium or restless melancholia, indicate such an extensive pathological change, that recovery is likely to come, if at all, only after a prolonged attack. An elevation of temperature to 100° or 101° also indicates a serious pathological condition, for it denotes a rapid metamorphosis of tissue in the cortex. I refer, of course, to non-inflammatory cases. Obstinate insomnia, which is overcome only by large and hazardous doses of hypnotics, is an indication of a serious molecular disturbance in the cortical cells, and the attack will be comparatively prolonged. While many of these cases can be safely guided through the attack at home, preparation must be made for an illness of several weeks or months in duration, and when this cannot be arranged at home, resort must be had to the hospital.

Frequently the environment of the home will be such as will prevent the patient from receiving suitable nursing, even when none of the above indications is present, and then his interests demand his removal to an institution. When in others, again, the mental symptoms are such that the environments of the home are an irritant, aggravating the disorder, prompt removal should be urged. It may in such a case be either to the home of a friend or to a hospital.

Excluding all of these cases, however, there will remain not a small modicum of cases where none of the foregoing indications exists, in which home treatment is not only permissible, but advisable. There is always serious shock to a sensitive organization on transference to an institution; and association with disturbed minds in many cases is by no means an unmixed good. We cannot evade, further, the social odium which such removal entails, and when it can be avoided the interests of the patient demand it.

Insane hospitals are, when well conducted, comfortable homes and afford protection and asylum, but I do not think that as medical men we can boast much of the strictly medical treatment which they afford. In some instances I have even known the statement to be made as a boast, that not more than three or four per cent. of the patients in a given asylum required any form of medical treatment. Now if this be true, it either means that the disorder of function in psychiatry does not mean a pathological tissue change, or it admits a hopelessness and helplessness against which I most earnestly protest.

The fact is that the chief consideration in insane hospitals thus far has been to provide a comfortable *home* rather than to bring about prompt recovery. I know whereof I speak, and it is because of this that I would urge the physician to make a careful study of the pathology, symptomatology and treatment of the early stages of acute insanity, to the end that they may better sub-

serve their patients' interests and pass more intelligent judgment on the treatment which institutions for their care should afford.

HYPEREMESIS GRAVIDARUM.

BY W. A. DICKEY, A. B., M. D., TIFFIN.

The achievements wrought in the domain of surgery during the past few years have so much engrossed the minds of a great part of the profession, as to almost completely overshadow the multiplicity of wants of the busy practitioner. There are few of us, I take it, whose work is so entirely surgical as not to find interest and instruction in the consideration and discussion of questions that come under the notice of that greater number of the profession, the general practitioner. His work, though less ostentatious, is equally as important and his daily duties bring him in contact with cases of greatest gravity, requiring not only careful discernment, but intelligent, well directed judgment. Possibly in no one line of professional work are these cases so frequently met with as in obstetric practice. This is a fruitful field for observation and investigation, and one much neglected. There are many points that are yet obscure and upon which as a result there is far from unanimity of opinion. This is eminently true of the subject of this paper.

It is with the hope of bringing about a full and free discussion of the points involved in those cases of incoercible vomiting in pregnancy which in spite of the fact that the whole gamut of medicinal remedies and gynæcological means are used, go on from bad to worse until the life of the prospective mother is threatened. Are there any therapeutic agencies on which we can rely? when and by what means, if at all, shall we terminate

pregnancy? These are all fruitful themes for discussion and except on the first, I fear we shall not agree.

On the threshold of the discussion we are confronted with the question, what is hyperemesis gravidarum? Probably we can better answer this by telling what it is not. Those cases of obstinate vomiting in pregnancy which can be controlled by therapeutic means, or by the correcting of co-existing maladies which operate reflexly and in this way start a train of nervous phenomena ending in vomiting, would not be classed as "uncontrollable vomiting." Dubois defines uncontrollable vomiting thus: "The vomiting of pregnancy is uncontrollable when it affects seriously the health of the woman and resists the judicious use of a certain number of remedies." A more satisfactory definition would be one in which not only the health, but the life of the woman was threatened. The health of the patient might for the time be seriously affected and yet the case not terminate disastrously. This fact should not be forgotten. There are those who believe uncontrollable vomiting to be a very rare occurrence. Compared with the greater number that can be controlled, this is true; but when we reflect on the number of deaths that occur from pernicious vomiting we can no longer close our eyes to the danger arising from this distressing malady. Dubois within a period of thirteen years met with twenty fatal cases. Guenoit, in a collection of one hundred and eighteen cases, reports forty-six deaths. Joulins gives one hundred and twenty-one cases with forty-nine deaths. This number is now very greatly increased, so that if an equal number of deaths were to occur from any other cause, the subject would long ago have been thoroughly studied and better and more decided ideas entertained concerning it and the best and safest means of relief.

In many of these cases the paroxysms of vomiting occur with wonderful frequency. Anything taken into the stomach, whether liquid or solid, is sufficient to excite

vomiting. In some of them even when the stomach is empty the retching will continue. The patient soon refuses food and the emaciation is rapid and profound, the woman in a few days being reduced to a mere shadow of her former self. Even if the paroxysms should cease for a time, they seem to return with renewed energy and vigor. This of itself will often bring on abortion, a state of affairs much to be wished for, as we then get rid of the primal cause of the trouble. The vomiting will often come on after conception (as in the case to be given later) and continue until abortion is produced, when the vomiting will cease; then again it will not assume a serious aspect until the later months of gestation.

Dr. Grailey Hewitt, of London, in a brochure recently issued on "Severe Vomiting during Pregnancy" holds that there is always a cause for the severe vomiting, which must be found and corrected, when it will cease. He gives as the most important causes, displacement of the gravid uterus, thickening or induration of the cervix, endometritis, adhesions from old inflammatory action, etc. I grant all this and yet in themselves they may be such important factors that before they could be removed death would ensue. There are cases in which no cause save the presence of the fœtus in utero can be found, as the following will illustrate.

Was called March 20th of last year to see Mrs. H., in consultation with Dr. Archer of North Baltimore. She is a multigravida and during her last pregnancy had suffered greatly from obstinate vomiting, which was finally arrested with ingluvin. She was now in the sixth or seventh week of her third pregnancy, and had been confined to her bed for two weeks, vomiting incessantly. Practically the whole list of remedies in such cases had been tried. As ingluvin had relieved her during her last pregnancy and had not been used this time, it was concluded to give it a second trial, but it proved worthless. Oxalate of cerium alone and combined with ingluvin were

given, but to no purpose; in brief, the entire gamut of therapeutical remedies singly and combined were used with negative results. Anything taken into the stomach, whether liquid or solid, was immediately ejected. Even when nothing was swallowed there was constant retching. An examination per vaginam was made, but nothing was found that could in any way aggravate the trouble. The uterus was freely movable and was in normal position. The os uteri, as suggested by Dubois, was dilated and a pledget of cotton soaked in glycerine was crowded well up against the cervix with the hope of lifting the uterus higher up in the pelvis, but this was of no avail. Applications of nitrate of silver and cocaine were made to the cervix, and the only effects noted were almost entire suppression of urine and the most intense strangury produced by the latter: two symptoms I have never heard mentioned as following the application of this drug. In the mean time no food was taken into the stomach, but was injected into the rectum as extolled so highly by Dr. Campbell. Cracked ice was used to allay thirst. Injections into the rectum of bromide of potassium as suggested by Dr. Bussey, of Washington City, were used with the same unfortunate results. Nothing gave any relief, except hypodermatic injections of morphine and atropia, and as soon as the effects of these would pass away in the least degree retching would commence. Cocaine was given internally but to no avail. Two weeks were thus consumed in a fruitless endeavor to relieve this woman of her suffering. She had now been confined to her bed four weeks retching and vomiting incessantly. She was greatly emaciated, the eyes sunken, features haggard, the pulse weak and rapid, usually 120 per minute, and the abdominal muscles markedly tender. She was now so much reduced that it was feared any untoward symptoms following an abortion would prove fatal, but to my mind it offered the only hope of relief.

After consultation and the facts stated to husband and

wife it wasdecided to relieve the uterus of its contents, which I proceeded to do in the following way:

The vagina was well washed out with warm carbolized water and a uterine sound previously made aseptic by carbolic acid and alcohol was gently inserted into the uterus, where it was allowed to remain for an hour; four hours subsequently it was again introduced, where it was allowed to remain for the same length of time. This was at six o'clock in the evening; at two o'clock in the morning I had the extreme satisfaction of knowing that the contents of the cavum uteri had passed away. The woman did not vomit again. She was given half a glass of hot milk and immediately fell asleep and slept until morning, when she was given some beef tea. Her recovery was uninterrupted.

I have thus at some length given the means adopted for relief in this obstinate case, to illustrate the fact that cases do occur in which no cause for the vomiting can be found, save pregnancy alone, and that absolutely nothing will end the sufferings of the unfortunate victim save the induction of an abortion. Prof. Hirst, of Philadelphia, whom we all admire so much, advocates a plan of procedure where this is to be done that I regard very highly and that I should follow again. After fixing the cervix with a double tenaculum, he dilates the canal with graduated bougies until it attains approximately the diameter of one's thumb; iodoform gauze is now packed into the cavity and a pledget of antiseptic cotton is placed into the vagina; these are allowed to remain until such time as uterine contraction demands their removal. Dr. Hirst recently reported to the Philadelphia Obstetrical Society four cases of hyperemesis gravidarum, in all of which except one it was necessary to induce abortion. In the first one of the series it had been done in a former pregnancy. The second one died three days after the operation, from inanition, the remainder recovered. In each of these, rectal alimentation had been used and

medicine given canonically. In all of these cases the strength of the patient was much reduced and the pulse was weak and rapid.

Abortion for the relief of obstinate vomiting in pregnancy was first done by Simmond in the year 1813. In 1852 it formed the subject of a very animated discussion in the French Academy of Medicine, but as might have been expected, such a diversity of opinion was expressed that no definite ideas could be formulated. It is not at all surprising then that when the great leaders of medical thought hold so radically antagonistic views the rank and file of the profession have so long hesitated in doing or recommending the operation. Indeed there are those yet who believe that such an operation is never justifiable; they have faith to believe that in some unknown and mysterious way nature or Providence will interfere, the vomiting cease and the life prospective saved. I am one of those who are unwilling to trust to either nature or Providence what is clearly my duty to perform in the premises, lest they be in other business and my patient suffer. Cazeaux says that under no circumstances ought such an operation to be done for the relief of vomiting. I am informed that Martin, of Berlin, holds the same views. Fortunately for suffering womanhood this idea is passing away, slowly though it may be.

Lusk in speaking of the subject says, "If the rectal method of nutrition fails to relieve the patient, the induction of abortion or premature labor should be resorted to before the stage of extreme inanition is reached." Muller says, "The induction of abortion is justifiable in all cases in which the continuance of the uncontrollable vomiting unmistakably threatens the maternal life." Parvin, "The true radical treatment of incontrollable vomiting in pregnancy is emptying the uterus." Hirst holds the same views.

There are those who will object to this proceeding because some die after the operation. Granted, but why

I am satisfied that in our anxiety to save a life that has only a problematical existence we jeopardize that of one which has an actual existence by waiting too long; in this I think we are culpable. Flaischler, in reporting five cases of pernicious vomiting, three of which perished, gives it as his belief that the uterus was emptied too late. Hirst says in his opinion if interference had come early enough the patients would not succumb.

Another objection that is sometimes urged is that it has a very deleterious effect on the woman's subsequent health. I do not regard this position as at all tenable. I think if we reflect on the number of women who have had one or more miscarriages we will find that quite as many are invalids who have never miscarried as there are those who have. The extreme optimist will argue that we have no right to perform an operation which has for its primal object the destruction of fœtal life, even though the ultimate object be to save one of vastly more importance. With such a belief I can hold no fellowship. If the mother dies, the child will die also, and thus we have lost by neglect two lives where if the fœtus is sacrificed the one of greatest consequence to family and community is saved. I am aware that this might be abused, and yet I am slow to believe that a conscientious physician after consultation and deliberation with a fellow practitioner would deliberately perform an act for which his own conscience, to say nothing of the laws of God and man, would condemn him.

Is there any symptom that will serve as a guide to indicate when the life of the woman is in danger? After a somewhat careful study of all the cases at my command there is one that seems to be constant, and that is a weak, rapid pulse. Flaischler refers to it in the cases reported by him and says he regards this "as the most important indication of danger." Prof. Hirst also speaks of the rapid, irregular pulse. It was present in the case referred to in this paper. If further observation shall confirm this, it will prove of great practical importance.

From the foregoing I deduce the following propositions: First: There are no medicines on which we can rely for the relief of hyperemesis gravidarum. Second: The injections of food into the rectum will not give relief. Third: The induction of abortion for the relief of incoercible vomiting is a legitimate operation, and should be done before the state of exhaustion sets in. Fourth: A weak, rapid pulse is an indication of danger. Fifth: It should never be done without consultation. Sixth: It should be done with antiseptic precautions.

TESTS FOR ALBUMEN.

BY WILLIAM B. DAVIS, A. M., M. D., CINCINNATI.

Among the many equipments required of the physician of to-day is that of making a thorough examination of the urine. No excuse for any deficiency in this direction will free him from the imputation of being behind the age. Albuminuria is so much more prevalent than it was formerly supposed to be—even among persons apparently healthy—that the interests of our profession, as well as the demands for examinations for life assurance, require physicians to be always prepared to make a urinalysis. At a recent meeting of the medical directors of the large assurance companies of America, the statement was made that if the medical examiners for life assurance were all qualified to make a reliable urinalysis of every person examined by them, the clinical significance of albuminuria in persons apparently healthy would soon be solved.

In a paper which I read before the American Medical Association held in Washington, May, 1891, on "The Prevalence of Albuminuria in Persons Apparently Healthy," I showed by reports of observers in the United States that albuminuria was found to prevail in from ten to twenty per cent. of all persons examined.

In Great Britain and in Europe larger percentages have been found. Prof. Grainger Stewart, in a report to the Royal Medical Society of Edinburgh, stated that he found albumen present in 31 per cent. of 407 urines from healthy individuals, and he concluded his report by say-

ing "that albuminuria is much more common among presumably healthy people than was formerly supposed, tests having demonstrated its presence in nearly one-third of the population."

Dr. Turner, Medical Officer of Essex County Insane Asylum, England, found albumen in the urine of 40.02 per cent. of 200 male inmates, and in another insane asylum, Kleudgen found albumen in 43 per cent. of the healthy nurses. Stirling found it in 44 per cent. of 461 healthy adults examined by him; Capitan found it in 45 per cent., and in children 89 per cent.; De la Celle-Chateauberg says it may be found at times in the urine of 76 per cent. to 100 per cent. of healthy persons, both young adults and children; Posner asserts that he has proven the existence of albumen in all normal urine, and his experiments have received the stamp of approval from such men as Senator, Duden, Leube and V. Noorden. Washburn, of Milwaukee, in *The Medical News*, April 5th, 1890, says that in order to verify the claim of Posner, he tested with picric acid by the contact method, and also with the phenic-acetic test, and with a solution of citric acid of a specific gravity of 1.008 by the same method, samples of urine from fifty persons in perfect health, all living in good sanitary surroundings, and "in every instance, at the line of contact of the two fluids, the characteristic cloud appeared with more or less distinctness, indicating the presence of an albuminoid."

Prof. Senator, of Berlin, in his recent work entitled "Albuminuria in its Physiological and Clinical Relations," Berlin, 1890, says: "The more complete methods of investigation, and the application of delicate tests have had as a result the discovery that albumen is frequently found in the urine of men who exhibit neither objective or subjective symptoms of disturbances of health, and who, after a long continued observation, appear to be perfectly healthy. From the reports of a number of the best authors," he says, "we are warranted in believing that albu-

men is found in the urine without any other discoverable pathological changes, in a surprising number of cases. Forty-one out of every 100 healthy, strong men, especially soldiers, under ordinary circumstances, have albuminuria."

Sir Wm. Roberts says it has been both affirmed and denied, on high authority, that traces of serum-albumen exist in normal urine. "I have satisfied myself," he says, "that concentrated urines from persons in undoubted health are comparatively rarely free from traces of albumen, detectable by direct testing. In ordinary processes of testing urine, these traces are naturally overlooked, but they certainly exist very frequently, and their existence shows how nearly on the verge of a sensible albuminuria healthy people are."

It is somewhat remarkable that the recorded observations of our country show a low percentage of albuminuria in persons apparently healthy when compared with those of the British and Continental authorities. I asked Dr. Victor C. Vaughan, Professor of Chemistry in the University of Michigan, why it was that physicians in Great Britain and Europe, men of undoubted ability and world wide fame, were finding albumen in the urine of 20, 30, 50, 70 and 100 per cent. of healthy persons examined by them, when the highest per cent. reported in our country was but from 10 per cent. to 20 per cent.? In reply to my inquiry he wrote: "I will agree to the statement that proteids are frequently found in the urine of the healthy, or those who, so far as they themselves or anyone else may know, are healthy;" and he called my attention to an article on "The Proteids of the Urine, with a Comparison of the Tests for Albumen," by F. G. Novy, M. S., Instructor in Physiological Chemistry, University of Michigan (*Medical News*, Vol. 53, 1883) and said: "You will see from this article, to which I have referred you, that with some of the proposed tests you can find albumen in the urine of

almost any one." In this article Mr. Novy calls attention to the fact that serum-albumen is the only proteid which the average physician expects to find in the urine, whereas, globulin, hemi-albumose (Bence Jones albumen) and peptones are also frequently present. In the ordinary methods for testing for albumen, all of these proteids but peptone may be readily mistaken one for another, and, indeed, he says, "this has been rather the rule than the exception." Serum-albumen is generally considered to be indicative of structural disease of the kidneys, but the other proteids have no such grave significance; nevertheless, serum-albumen is usually accompanied by larger or smaller quantities of globulin, and in some instances even peptones may be present; "our ordinary tests for serum-albumen are also responded to by globulin, and hence, when applied to an albuminous urine, they indicate as a rule, a greater amount of serum-albumen than that actually present."

Globulin usually accompanies serum albumen, and may be present in almost all varieties of albuminuria, though in variable quantity. The more recent investigations of Maguire indicate that globulin can exist in the urine by itself, even without serum-albumen being present. Novy mentions one case of functional albuminuria which came under his observation where globulin alone was found in the urine. These reports accord with the statement I have made in my paper on "Functional Albuminuria," read before the American Medical Association, May 22d, 1890, viz.:—

"The present line of investigation is pointing strongly toward globulin as the form of albumen which is likely to be found in the various forms of functional albuminuria when chemistry will furnish us with simpler methods and more reliable reagents for its discovery."

Of late years there has been a great multiplication of chemical tests for the discovery of albumen in the urine. These tests are becoming so delicate that an infinitesimal

quantity can be detected by some of them. Indeed the claim is made for one of them that it will detect less than one part in 1,000,000 of water, while Posner's test, it is claimed, reveals albumen in all normal urine.

Senator, in his recent work, publishes the following as Posner's test:—"Add to the filtered urine three times its volume of alcohol, or a concentrated aqueous solution of tannin, wash out the precipitate with water, and then dissolve with acetic acid. Or add a large quantity of acetic acid to the urine, and then evaporate in order to concentrate the urine. In both cases, in the acetic acid solution, all the tests for albumen which are not influenced by the acetic acid, will give a positive result."

Mr. Novy has shown that many of these tests act on the other proteids of the urine as well as the serum-albumen, and Prof. Thos. R. Fraser, of Edinburgh, says "some of them produce changes in the urine, which are not necessarily dependent on the presence of albumen, and hence are liable to erroneous interpretation, and albumen may be supposed to exist in urine which is actually free from it." There is no doubt but that some of the tests recently introduced are useless, and others are difficult of application, and some are misleading. In order that there may be uniformity in our investigations, and a significance to our reports, there should be an agreement by chemists and the medical officers of life insurance companies upon certain tests for albumen in the urine, which will be recognized as standard tests.

In answer to my inquiry, as to which he considered the best tests, Prof. Vaughan, of the University of Michigan, writes: "The only tests which distinguish the albumens from the albumoses and peptones, are, I think, the heat and nitric acid tests, the nitric acid contact test, and the acetic acid and potassium ferrocyanide test."

Sir Wm. Roberts says the best tests for albumen are coagulation by boiling, and nitric acid; in doubtful cases, the two tests should be used in succession.

Saundby says, I have long maintained the supremacy of boiling and acidulating with dilute acetic acid.

Pollock ranks pure nitric acid, half an inch deep in the bottom of a clean test tube, the urine to be poured gently on, as the best test. Next in rank is boiling, the urine to be previously acidulated.

Goodheart believes nitric acid to be the best form of test for albumen occurring in the urine.

Pye Smith said that as the medical officer of a London Assurance Society, he had tried a great many tests for albumen in the urine, and he was driven back to the old tests of heat and nitric acid, as the most satisfactory of them all.

Tyson gives the first place to the "test by heat," and the second to nitric acid by the contact, or Heller's method.

Prof. Fraser advocates the adoption of one system of testing, in which the test is sufficiently delicate, and at the same time free from objections; with this object in view, as the chief medical officer of the Standard Life Assurance Company, of Edinburgh, he recommends the invariable adoption by the medical officers of the company of the well known test of boiling a small quantity of the the urine in a test tube, then adding a drop or two of concentrated nitric acid, and again boiling.

George Johnson regards picric acid "as the most delicate and entirely trustworthy test for minute quantities of albumen." Novy says this reagent, picric acid, "cannot be said to be as delicate as either warming, with subsequent use of nitric acid, the nitric acid and contact method, or acetic acid and potassium ferrocyanide, and moreover, the number of normal urines with which it gives reactions is quite considerable."

Goodheart, Tyson, and other authorities have observed that urates, mucin, peptones, alkaloids, as quinine, morphia, etc., are all precipitated by picric acid.

From this testimony it is evident that our old tests,

with which we are all familiar, viz.: heat and nitric acid, may still be regarded as the best and most reliable reagents for the detection of albumen in the urine.

The refinement of our chemical tests, however, and the fact that some of them give reactions with other proteids than serum-albumen, do not account for the wide discrepancy between the American and the British and the Continental reports of the prevalence of albuminuria in persons otherwise healthy. The only satisfactory explanation to be made is that our professional brothers across the sea have given more attention to the subject, and have been more systematic in their observations and exact in their reports than we have been.

DISCUSSION.

Dr. John North:

In regard to the tests for albumen, I do not think that we have any better tests than heat and nitric acid; nitric acid may cause a turbidity of the urine when albumen is not present, if there is an excess of uric acid or urea in the urine, and if the patient has been taking some of the alkaloids. It has been found that in the urine of a patient taking quinine and other alkaloids there will be a precipitate very much resembling albumen. There is another test which I use, because the presence of other substances sometimes found in urine does not have any effect upon it. It is simply ordinary vinegar, saturated with common table salt. It is one that can be used at any place, very easily, and made as quickly, and is as delicate, if not more so, than the nitric acid test. Simply take the vinegar and put as much table salt in it as it will dissolve, put a small portion of the solution in your test tube, bring it to the boiling point, and pour your urine on top, and it will produce a white zone if there is a trace of albumen in it. It is a test you can make in the country, using an ordinary tumbler or any convenient vessel. I received a report from a professor in chemistry, who reports to me that it is the most delicate test that he has ever used.

As regards the presence of albumen in healthy persons: I have had albumen in my urine for the last eleven or twelve years. I discovered it accidentally while lecturing to a medical class. I submitted a portion of my urine

to test as a specimen of healthy urine. I found, to my astonishment, albumen in considerable quantities. I went to the laboratory, and examined my urine, and found from 33 to 50 per cent. of albumen by bulk. I submitted that urine to the heat and nitric acid test. Albumen has been there ever since. There has not been less than 10 per cent. of bulk and usually about 20 per cent. At that time and previously I was in perfect health. Before that time I never weighed over 142 pounds; I now weigh 193 pounds, and am in perfect health. I went to Philadelphia, and consulted specialists with regard to my condition. They gave me six months in which to die. I went home to Iowa and got ready to die, so far as I could, but didn't die. I have lived a good many years since. Since that time I have been gaining in flesh, and have been feeling better all the time. I believe that doctors sometimes make mistakes. I do not think that I am going to die with Bright's disease. Albuminuria is one of the symptoms of Bright's disease, but not necessarily Bright's disease.

Dr. Dixon:

I have in my family and in my employ a man who has had albumen in his urine for over two years, and also renal casts, in which the amount of the albumen and the amount of the renal casts are both disappearing. His general health has been improving steadily for one year. I would like to ask the doctor who was last up, what is the microscopic analysis of his urine, so far as casts are concerned.

Dr. North:

No casts.

OBITUARY.

DR. BUSHROD W. DAVIS.

The subject of this sketch was born in Loudoun county, Virginia, February 6, 1813, and with his parents settled in Claridon township, Marion county, Ohio, in 1830. He was educated in the common schools of that day, and acquired sufficient education to enable him to teach school, which he did until 1849. In 1835 he moved to Belmont county, Ohio, where he studied medicine and began to practice in 1848. On November 23, 1837, he married Miss Harriet Hatch, a native of that county. In 1855 he located at Osborn, Greene county, Ohio, and practiced two years. From there he moved to Caledonia, Marion county, Ohio, in 1857, and in 1860 permanently located in Marion, Ohio. He was one of the founders of the Marion County Medical Society, and served as its president and secretary a term each, subsequently was elected an honorary member of the same. He was a member of the Ohio State Medical Society, and was elected one of its vice-presidents at the session held at Columbus in 1884. In 1876, he received an honorary degree of Doctor of Medicine at Cleveland, Ohio. In 1862 he performed the duty of examiner of soldiers under the appointment of Governor David Tod, of Ohio, and was a member of the Board of Examining Surgeons of Pensioners under the administrations of Presidents Garfield and Arthur.

He became a member of the Methodist Episcopal Church in 1839, and retained his membership in that church until his death. He died at his home in Marion, Ohio, at one o'clock, Thursday morning, April 16, 1891, at the ripe age of seventy-eight years, surrounded with plenty, and a host of admiring friends. During his professional career he was held in high esteem by his professional brethren who had the pleasure of his acquaintance.

ARTHUR B. CARPENTER, M. D.

Dr. Arthur B. Carpenter of this city died suddenly Wednesday morning, October 15, 1890. He had been enjoying his usual health, and on Tuesday evening was dining with some friends at the Union Club. About eleven o'clock he remarked that he was not feeling well, and went to his rooms accompanied by Dr. Corlett. Upon reaching his office, which was next door to the club house, he remarked that he was feeling very sick and requested that Dr. Kitchen be sent for. He complained of suffering much pain; his pulse became imperceptible, which, after vomiting, returned for a short time. Notwithstanding hypodermic injections of brandy and ether, he sank rapidly and expired in less than two hours. He retained consciousness almost to the last, and realized fully his serious condition and made disposition of his property.

Dr. Carpenter was born at West Andover, Ohio, in 1852. He studied medicine in the office of Dr. Kitchen; graduated from the Medical Department at Wooster University in the year 1879, after which he served one year in the Cleveland Hospital for Insane, did a general practice in this city for several years, and went to Europe and spent a year in the hospitals of London and Berlin, returning in 1887, since which time he limited his practice to diseases of women. For several years he occupied the position of Demonstrator of Anatomy in the Medical Department of Western Reserve University.

Dr. Carpenter was a man of fine physique and prepossessing appearance, which, together with the strong personality, left a lasting impression upon all who came in contact with him.

He was a member of the British Gynæcological Society, and at the last meeting in Buffalo was elected a member of the American Gynæcological Society. He was vice-president of the Cuyahoga County Medical Society, also a member of the Union Medical Association of Northeastern Ohio, the Ohio State Society and the American Medical Association. He was an active member, always present and taking a prominent part in the discussions, and during the past few years read a number of valuable papers before the meetings. He has been a frequent contributor to the *Medical Gazette*, as well as other medical periodicals. He will probably be best remembered by the profession in connection with Apostoli's method of treating uterine fibroids, and as having been the first to make use of the ordinary street electrical wires for medical purposes.

At a special meeting of the Cuyahoga County Medical Society, Thursday evening, October 16, a committee consisting of Drs. Corlett, Humiston, Lee and Vance was appointed to draft resolutions, and submitted the following, which was unanimously adopted:

In view of the death of Dr. Carpenter, we, the committee of the Cuyahoga County Medical Society, would respectfully submit the following: That a man of more innate kindness and frankness of disposition never lived; that Dr. Carpenter, in the work he did in his chosen lines, was original in thought, apt in suggestion and skillful in execu-

tion, and in his death we mourn the loss of a congenial companion, a careful student and skillful physician; that an engrossed copy of these sentiments be sent to the family of deceased, the city papers, and inscribed on the minutes of this society.

During the meeting, eulogistic remarks about the life, character and work of Dr. Carpenter were made by Drs. Scott, Cushing, Tuckerman, Baker, Powell, Ashmun, Corlett, Humiston, Lee and Vance.

During the course of his remarks, Dr. Herrick said:

"Dr. A. B. Carpenter has been so conspicuous among us and so ready in all the questions of interest to our profession, that it becomes us to stop and learn the lesson before us and drop a tear of sorrow over our lost comrade, and extend words of sympathy to those who have lost one in whom they had the most lively hope. Dr. Carpenter was no ordinary man. He had such natural endowments as gave him conspicuous success for his years, under opposing circumstances. Few know of the wearying struggle, the discouragements to be overcome by one who has risen from the humbler walks of life on a farm to the position attained by our cordial, generous fellow-worker. His personal appearance was commanding, his genial manners captivating. No one who had met Dr. Carpenter could fail to admire his frank, open expression of his convictions, which he did not fear to express or follow. He followed them without malice to those who chose to differ from him.

"In his work as demonstrator of anatomy, he

made no mean reputation. Rarely, if ever, in the city, has a demonstrator done as thorough work. By the aid of a magic lantern, he presented upon canvas a picture of complex parts to be dissected, and gave the students thorough drills with repetition, so that the points were fully impressed. In his more special work for the last three or four years, that of gynæcology, he did valuable service. Whatever he did he did with all his heart, and whatever he believed he believed with all his heart. He had strong purpose, accompanied with a commanding physique and a large brain force. With these endowments, in the order of Providence, had he lived, he would yet have gained still greater success and given greater luster to our noble profession."

In accordance with his often expressed wish while living, his remains were cremated in Buffalo on Tuesday, October 21.—*Cleveland Medical Gazette.*

DR. I. L. DRAKE

Was born in Warren county, Ohio, in June, 1823, and died at his home in Lebanon, Ohio, Sabbath morning, March 1, 1891, of heart disease, in the midst of his usefulness. He had been subject to attacks of neuralgia of the heart for a number of years and had frequently expressed the opinion that one day it would prove fatal, but that did not deter him in his zeal for his chosen work, which he carried on faithfully and lovingly to the end. Notwithstanding this, his death was sudden and unexpected to his family and friends and a shock to the community.

He was a son of Lewis Drake, a prosperous farmer who settled near Lebanon in the early part of this century. His mother was a Lincoln, and a relative of Abraham Lincoln.

After receiving an academic education, he graduated in medicine from the Miami Medical College, Cincinnati, and afterward attended lectures in a leading medical school in Philadelphia. He commenced the practice of his profession at Ft. Ancient, Ohio, and later removed to Lebanon, where he continued to reside until his death. He was an eminent physician, a good practitioner, with an extensive practice, steadily and studiously devoted to his profession, especially to surgery. For over thirty years he has been one of the most enthusiastic members of the Lebanon Medical Society, of which he was for a long time president.

Dr. Drake was a prominent member and ardent

supporter of the Baptist church, with which he had been connected since 1837, an active, conscientious worker in the church and in all Christian and moral efforts, and held a high position in religious, professional and social circles. He leaves a wife and six children to mourn the loss of a loving and devoted husband and father. And let friends and family all know that his name will be treasured tenderly and "that his memorial shall not depart away."

DR. E. W. HOWARD

Of Akron, Ohio, was born at Andover, Vermont, on April 14, 1816, and died August 9, 1890, aged 74 years, 3 months, 24 days.

Death was the result of a severe attack of la grippe in February preceding, from which he never fully recovered.

Graduated from Berkshire Medical College, Pittsfield, Massachusetts, in 1838, and practiced his profession in Akron since 1839.

Was a member of American, Ohio State, North-Eastern Ohio, and Summit County Medical Societies.

J. H. Seiler,
Secretary.

DR. W. J. UNDERWOOD,

Of Akron, Ohio, was born at Dillsburg, York Co., Pa., on March 20, 1840, and died June 9, 1890, aged 50 years, 2 months and 19 days, from Bright's disease.

Graduated from Jefferson Medical College in 1864, and practiced his profession in Akron since 1867.

Was a member of American, Ohio State, Northeastern Ohio, and Summit County Medical Societies. Was also President of Board of Pension Examiners of Akron.

J. H. SEILER,
Secretary.

DR. FOREST W. BRAYTON.

The year just passed has taken from us quite a number of our profession and members of our society. Not only has the grim reaper touched those full of years, but death has come to the middle aged and young. We as a society are called upon to pay tribute to the memory of one of the most promising of our members, one who was full of promise, a student, a brilliant practitioner, a genial gentleman and a peer of any. I refer to the lamented Dr. Forest W. Brayton, who died under such distressing circumstances, December 4, 1890.

Forest W. Brayton was born at McCutchenville, Ohio, September 10, 1856, his father, Dr. Asa Brayton, an honored member of this society, and his mother, Emily J., formerly Hutchens.

Having pursued his primary education he attended Miami Medical College, from which institution he was graduated in the spring of 1880.

Dr. Brayton was associated in the practice of medicine with his father at Carey, Ohio, from 1880 until January, 1890, when he moved to Toledo, where he formed a partnership with Dr. W. W. Jones, remaining there until shortly before his last sickness. Dr. Brayton was secretary of the N. W. O. M. A. for two years, filling the office with great credit to himself.

In testimony of our friendship for him and as an expression of the loss the Ohio State Medical

Society sustains in his early death, the foregoing expression is ordered upon the minutes of our society.

D. W. STEINER,

Secretary N. W. O. Medical Association.

DR. ANSON R. SMART.

There is no profession in which the ties of fraternity and colleagueship are as strong as in the ranks in which we serve.

The character of our work and our relation to the public foster this feeling.

As the hardships and anxieties of military service seal the bonds of good fellowship between comrades in arms, so do the trials peculiar to our profession cultivate the fraternal spirit.

It is essential to true success.

When we look in retrospect over the life of Dr. Anson R. Smart, what features of our association with him have left the strongest impressions upon our minds?

He was gifted with a keen intellect, and had a mind well stored with philosophy and science.

His counsel and practice were fully abreast the most advanced teachings of the day.

But above all do we not cherish the memory of a man whose nature was the embodiment of good fellowship, one whose disposition was free from any taint of jealousy?

Yes, he was one of those large minds that deal with the events of this life in a truly philosophical spirit.

Though our association with him was brief, yet we may all feel that to have known and to have been in contact with such a man, was indeed a privilege.

Individually we will miss his genial companion-

ship, and as an Association regret to hear no more his instructive and entertaining discourses.

Committee Toledo Medical Association.
D. P. CHAMBERLIN.
S. S. THORN.
THOMAS HUBBARD.

Anson R. Smart was born at Adrian, July 4, 1841, and was, at the time of his death, 50 years old.

He studied medicine in the office of the Drs. Hale, in Hudson, Mich., in 1864, and was graduated from Long Island Hospital Medical College in 1872. Many people remember the tall, ungainly youth that was wont to emerge from their office; but the young student had the element of success in his strong forces of will, and became one of the first and most honored citizens of the town, being twice Mayor, as well as a leading member of the Board of Education, prominent in civic societies, and a promoter of local enterprises, notably in the musical and histrionic arts.

His ability brought him to the front among the medical fraternity. He was made President of Southern Michigan Medical Association, and also Regent of the Detroit Medical College, and member of the Lenawee county examining board of pensioners.

He sought a larger field for his capabilities in Toledo, removing to that city in 1886. His success followed him there, and he acquired an extensive practice. He was made Dean of the faculty of the N. W. Ohio Medical College, as well as the President of the Toledo Medical Association. His brother physicians, of that city, bear witness to his skill and ability, and particularly speak of his being the leading spirit in their

assemblages. His patients had an enduring confidence in him, and felt safe in his hands.

He was married to Ione, daughter of Dr. L. G. Hall, of Hudson, and his son Louis is now seventeen years of age.

His parents, Mr. and Mrs. Rositer Smart, and sister, Agnes, are residents of Hudson, and his brother, Dr. W. N. Smart, is located at San Diego, Cal.

DR. JOHN W. McMILLEN.

In his death Columbus loses one of her most prominent and valuable citizens, and a man of progress and advanced ideas. His reputation for honest, upright intercourse with his fellow-men was unimpeachable, and his integrity beyond question. Possessing all the attributes of a refined, courteous gentleman, he drew around him a large circle of friends whose friendship was valuable and by him retained to the last. He had attained to a plane of eminence in his profession seldom reached by one so early in life.

The immediate cause of his death was Bright's disease.

Dr. McMillen leaves a wife but no children.

John W. McMillen was born in Sunbury, Delaware county, September 13, 1849, and was at the time of his death 42 years of age. His parents were Scotch-Irish, and of a race that has left a lasting impress in America. Dr. A. S. McMillen, the father of the deceased, was a successful physician, with a large practice. The son was educated in the Ohio Wesleyan University at Delaware and in Denison University at Granville. He studied medicine in Michigan University, Ann Arbor, and in Long Island College Hospital, New York, where he was graduated and received the degree of M. D., June 23, 1873. He began practice with his preceptor, Dr. John W. Russell, of Mt. Vernon, and continued with him until November, 1886, when he came to Columbus. In April, 1888, Dr. McMillen was elected super-

intendent of the Columbus Asylum for Insane, and entered upon the discharge of his duties early in May following. He discharged his responsible trust as the head and excutive officer of this, one of the largest asylums in the world, to the great benefit of the State and of the patients intrusted to his care. The improvement in administration and the success of Dr. McMillen's work were marked and salutary. Dr. McMillen was a member of the Central Ohio and Ohio State Medical Societies, of the American Microscopical Society, of the American Medical Association and of the International Medical Society. He was married to Miss Alice Buckland, of Mt. Vernon, December 28, 1875.

LIST OF MEMBERS

OF

THE OHIO STATE MEDICAL SOCIETY,

WITH THEIR

POST-OFFICE ADDRESS AND DATE OF ADMISSION.

The names marked with an asterisk (*) have been members thirty years, and are exempt from payment of dues. Those marked † have deceased since last report.

Adams, P. N	Dayton	1885
Alcorn, A. W	Ravenna	1870
Allen, D. B	West Liberty	1865
Allen, Dudley P	Cleveland	1885
Allen, Harlan P	Columbus	1884
Amidon, Rebecca S	Painesville	1886
Anderson, James	Salem	1884
Andre, O. C	Piketon	1888
Asbury, T. B	Jerome	1878
Ash, William N	Toledo	1887
Axline, C. Z	Fultonham	1884
Ayers, S. C	Cincinnati	1877
Baker, A. R	Cleveland	1886
Baker, George G	Akron	1884
Baker, J. P	Findlay	1891
Baker, W. O	Louisville	1882
Baldwin, J. F	Columbus	1875

Ballinger, J. B	Versailles	1880
Barnes, E. J	Hebron	1888
Barnes, T. S	Chillicothe	1890
Barnes, W. T	Fredericksburg	1891
Barnhill, J. U	Columbus	1884
Barrere, D. M	Dayton	1884
Barton, T. J	Zanesville	1881
Bates, F. L	Lima	1890
*Battles, W. S	Shreve	1858
Bauer, M. M	Lake	1884
*Beach, J. N	West Jefferson	1854
Beach, T. D	Catawba	1884
Beardsley, C. E	Ottawa	1869
Beck, J. S	Dayton	1879
Beers, A. M	Newcomerstown	1882
Belt, W. A	Kenton	1887
Benner, C. T	Tiffin	1891
Bennett, W. C	Iberia	1891
Bennitt, J	Cleveland	1869
Bessey, J. Mortimer	Toledo	1887
Blair, James F	Linwood	1884
Blake, W. H	Shadeville	1878
Bleile, A. M	Columbus	1882
Boerstler, G.W	Lancaster	1870
Bonner, Horace	Dayton	1890
Boone, D. W	Bellaire	1890
Bowen, A. H	Columbus	1878
Boylan, A	Milford Centre	1878
Bramble, D. D	Cincinnati	1870
Brannan, T. H	Canal Dover	1884
Brashear, B. B	Akron	1886
*Bricker, W. R	Shelby	1848
*Brinkerhoff, D. H	Fremont	1853
Brockett, A. J	Cleveland	1874
Brown, H. H	Lyons	1887
Brown, H. M	Hillsboro	1882
Brown, J. A	Germantown	1886

Brown, J. S	Mecca	1889
Brown, Warren Bateman	San Luis Obispo, Cal	1888
Brundage, A. H	Xenia	1878
Brush, E. C	Zanesville	1878
Buckingham, John M	Springfield	1885
Buckner, J. H	Cincinnati	1863
Buechner, W. L	Youngstown	1879
Buffett, Lewis	Easton, Maryland	1880
Bunce, W. C	Oberlin	1891
Bunn, J. W	West Union	1872
Butler, G. O	Cleveland	1870
Butler, G. W	Columbus	1869
Caldwell, Wm	Fremont	1886
Caldwell, W. W	Hamilton	1874
Calvin, J. H	Huron	1891
Campbell, John	Galion	1891
Campbell, W. A	Eaton	1888
Carmichael, W. A	Loveland	1871
Carpenter, Julia W	Cincinnati	1879
Carrick, A L	Cincinnati	1871
Case, F. D	Ashtabula	1886
Chamberlin, D. P	Toledo	1874
Chaney, H. L	Columbus	1886
Chapman, Byron	Copley	1885
Chapman, W. C	Toledo	1875
Christopher, W. S	Cincinnati	1888
Clark, C. F	Columbus	1885
Clark, John H	Mechanicsburg	1869
Clark, M. S	Youngstown	1879
Clarke, Ida	Youngstown	1889
Cline, J. W	Germantown	1886
Cobb, Charles	New Athens	1889
Coldham, James	Toledo	1884
Coleman, N. R	Columbus	1875
Collamore, G. A	Toledo	1880
Collins, H. O	Dayton	1889
Collins, M. H	South Charleston	1890

Colvill, G. H	Harrisville	1887
Conard, G. R	New Vienna	1890
Cone, J E	Youngstown	1889
Conklin, W. J	Dayton	1868
Conn, Eli	Akron	1887
Conner, P. S	Cincinnati	1868
Cook, G. F	Oxford	1885
Cooper, A	Columbus	1881
Corlett, W. T	Cleveland	1882
Corson, O. M	Middletown	1879
*Cotton, J. D	Marietta	1852
Courtwright, A. P	Circleville	1871
Courtwright, G. S	Lithopolis	1867
Cowan, Chas. S	Hardin	1885
Cox, J. M	South Salem	1877
Craig, J. Harvey	Mansfield	1886
Craig, J. W	Mansfield	1874
Crane, A. M	Marion	1881
Crossland, J. C	Zanesville	1890
Cullen, G. I	Cincinnati	1890
*Cushing, H. K	Cleveland	1857
Dandridge, N. P	Cincinnati	1871
Darby, F. H	Morrow	1882
Davidson, N. P	Hilliard	1890
Davies, W. C	Granville	1882
Davis, A. A	Abilene, Kansas	1884
Davis, Wm. B	Cincinnati	1865
*Dawson, W. W	Cincinnati	1853
Deahofe, S. P	Potsdam	1888
Denman, W. C	Marion	1882
DeVilbiss, Allen	Toledo	1887
Dickey, Thomas A	Middletown	1879
Dimmick, O. A	Chardon	1884
Dimond, Henry C	Springfield	1885
Dixon, W. A	Ripley	1889
†*Donahoe, H. J	Sandusky	1854
Donnellan, J. E	Germantown	1879

Dorman, W	Harbor	1886
Downs, S	Waterville	1876
Duff, J. S	Cadiz	1888
Duncan, A. M	Bucyrus	1886
Duncan, James A	Toledo	1874
*Dunlap, Alexander	Springfield	1854
Dunlap. C. O	McArthur	1881
Dunlap, C. W	Springfield	1869
Dutton, C. F	Cleveland	1880
Dwyer, R. A	New Petersburg	1879
Eckman, Hiram	Tuscarawas	1869
Eddy, C. W	Marietta	1888
Eichberg, Joseph	Cincinnati	1882
Elster, L. A	Jeffersonville	1884
Ervin, N. B	Gibsonburg	1891
Erwin, R. W	Bay City, Mich	1872
Evans, A. E	Columbus	1882
Everhard, N. S	Wadsworth	1870
Everts, Orpheus	College Hill	1885
Fackler, J. E	Versailles	1879
Ferguson, J. W	West Salem	1882
Ferrell, C. B	Columbus	1878
Finley, W. H	Spring Valley	1890
Fisher, H. M	Akron	1881
Fisher, P. Q	Nashport	1873
Fitzpatrick, T. V	Cincinnati	1890
Fleming, Ellis	Wingett Run	1889
Floyd, J. C. M	Steubenville	1884
Follett, Alfred	Granville	1864
Foltz, Abner E	Akron	1886
*Forbes, S. F	Toledo	1856
Foster, G. F	Kingston Center	1882
Foster, C. A	Washington C. H.	1891
Fowler, S. W	Delaware	1873
Frankenburg, O	Columbus	1873
Fraunfelter, James	Canton	1886
*Fuller, S. W	Bellefontaine	1853

Galbraith, E. J.....Chillicothe 1876
Gale, C. Bernard.....Newport 1886
Gale, G. T.....Newport 1888
Galer, Frank M.....DeGraff.....1874
Galloway, C. M.....Xenia.....1888
Galloway, W. A.....Xenia.....1890
Gawne, A. J .....Sandusky 1870
*Gay, N.....Columbus 1848
Gibbon, H. B.....Tiffin 1891
Gibson, Richard D .....Youngstown 1882
Gillespie, J. M.....Bloomingburg 1880
Gilliam, D. T.....Columbus 1873
Gilliam, E. M.....Columbus 1890
Goodhue, George.....Dayton 1884
Goodson, J. W.....Sterling, Kas 1874
Gordon, Joseph C.....Mt. Vernon 1886
*Gordon, T. W.....Georgetown 1849
Goss, J. H.....Lancaster 1884
Graefe, Charles.....Sandusky 1891
Graefe, Wm .....Sandusky 1874
Graham, W. T.....Lebanon 1888
Granger, B. D.....Russell.....1874
Granger, F. M.....Russell 1881
Gray, F. C .....Lewisburg.....1879
Green, H. S.....Cardington 1878
Greene, D. W.....Dayton.....1882
Griswold, E.....Sharon, Pa 1889
Groves, J. E.....Uhrichsville.....1884
Guerin, L. T.....Columbus 1888
Guerin, Z. F.....Columbus 1884
Guittard, A. M .....Killbuck 1886
Guittard, F. J.....New Bedford 1875
Gunsaulus, F.....Columbus 1886
Haldeman, J. S.....Zanesville.....1885
Hall, C.....Burlington 1872
Hall, R. B.....Cincinnati 1884
Hall, W. W.....Springfield 1882

Hamilton, C. S	Columbus	1891
Hamilton, H. A	Perrysburg	1886
*Hamilton, J. W	Columbus	1849
Hamilton, Wm. D	Columbus	1886
Hammel, M	Circleville	1878
Hardy, J. M	Whipple	1886
Hare, G. A	Mt. Vernon	1889
Hare, Jessie D	Mt. Vernon	1889
Harmon, F. M	Cleveland	1889
*Harmon, Julian	Warren	1857
Harrison, Eugene B	Napoleon	1869
*Hart, B. F	Marietta	1854
Hart, Samuel	Marietta	1887
Hartman, G. W	Archbold	1887
Hartman, S. D	Tippecanoe City	1874
Hasencamp, O	Toledo	1891
Hathaway, H	Toledo	1874
Hedges, W. B	Delaware	1878
Heistand, E. B	Kenton	1884
Heiter, Carl	Sandusky	1875
Hellbish, F. S	Fremont	1878
Helmick, G. W	Harrisburg	1878
*Henderson, D. W	Marysville	1856
Henderson, R	Urbana	1888
Hendrickson, Hugh	Columbus	1868
Herrick, H. J	Cleveland	1868
Hiddleson, C. S	Atwater	1888
Hill, H	West Alexandria	1867
Hill, N. S	Neville	1866
Himes, Isaac N	Cleveland	1877
Hinckley, H. D	Oxford	1887
Hiner, S. B	Lima	1874
Hines, J. A	Van Wert	1879
Hipple, J. R	Waldo	1881
Hise, A. H	New Washington	1890
Hixon, M. M	Dupont	1887
Hobbs, W. A	Mineral Ridge	1889

Hobson, J. A	Flushing	1891
Hodges, G. H	Urbana	1888
Hoff, J. W	Pomeroy	1872
Holliday, B. W	Cleveland	1875
Hoover, C. M	Alliance	1885
Hough, C. A	Lebanon	1890
Hough, W. S	Cuyahoga Falls	1875
House, A. F	Cleveland	1880
Hubbard, Thos	Toledo	1889
Hughes, M. A	Port Clinton	1877
Humbert, Frank	Mt. Liberty	1890
Humiston, W. H	Cleveland	1880
Hunter, T. S	Ashland	1878
Hurd, A	Findlay	1870
*Hyatt, E. H	Delaware	1862
Iddings, A. H	Dayton	1879
Ikirt, G. P	East Liverpool	1882
Ireland, W. E	Washington C. H	1887
Jacobs, W. C	Akron	1881
James, A. D	Mt. Gilead	1891
Jennings, E	Dayton	1867
Jewett, H. S	Dayton	1871
Jewett, W. H	West Canaan	1869
Johnson, O	Worthington	1878
Jones, John D	Cincinnati	1887
Jones, J. D	Newburgh	1870
Jones, R. E	Gomer	1863
Jones, Toland	Columbus	1878
*Jones, T. W	Columbus	1847
*Jones, W. W	Toledo	1848
Judkins, C. P	Cincinnati	1887
Kahle, A. W	Lima	1887
Kahle, R. D	Lima	1887
*Kay, Isaac	Springfield	1853
Keller, B. T	Bloomingburg	1891
Keller, Thomas F	New Washington	1887
Kelly, H. R	Galion	1868

Kelly, W. J.	Galion	1881
Kemper, A. C	Cincinnati	1871
Kendig, E. V	Hayesville	1874
King, C. W	Dayton Insane Asylum	1881
King, John I	Burgh Hill	1884
Kinkead, W. W	Nashville, Tenn	1884
Kinsman, D. N	Columbus	1864
Kirkley, C. A	Toledo	1874
Krause, B	Cleveland	1880
Kreiger, G. L	Lebanon	1884
Kurz, C. E	Bellaire	1875
*Landon, C. P	Westerville	1851
Larimore, F. C	Mt. Vernon	1871
Lash, H. M	Indianapolis, Ind	1879
Lash, J. M	Chillicothe	1879
Lathrop, A. B	Swanton	1887
Lawless, J. T	Toledo	1887
Lawrence, F. F	Columbus	1888
Leach, S	Mt. Sterling	1888
Leahy, Maurice	Tiffin	1891
Lee, E. B	Garrettsville	1870
Lee, M. F	Columbus	1888
Lefevre, Edwin	Sidney	1891
Levan, D. H	Mt. Victory	1879
*Leonard, B. B	West Liberty	1854
Leonard, B. S	West Liberty	1882
Leonard, W. W	Delaware	1891
Leslie, J. M	Chillicothe	1878
Lewis, W. C	Rushville	1869
Lightner, S. B	Sabina	1888
Love, M. J	Bloomingville	1891
*Loving, S	Columbus	1858
Lowman, J. H	Cleveland	1877
*Lyman, C. N	Wadsworth	1852
McCandless, M. D	Youngstown	1889
McClellan, B. R	Xenia	1888
McClintick, Samuel	Mt. Sterling	1890

McClung, Jno. C.....Leipsic 1887
McClure, James.....Marietta 1866
McCollum, E. J.....Tiffin 1871
McCurdy, John .....Youngstown 1870
McCurdy, S. L.....Dennison 1882
McDowell, John O.....Delaware 1884
McEbright, T .....Akron 1866
McGavran, S. B.....Cadiz 1887
McGee, J. B.....Cleveland 1880
McGlenn, J. A .....Mt. Pleasant.....1891
McGrew, Henry .....Pleasant Ridge.....1878
*McIlhenny, J. J.....Dayton 1847
McIntyre, W .....Warrensburg 1881
McKee, E. S.....Cincinnati 1886
McKim, F. E.....Marietta 1886
McKinley, E. B.....Vaughnsville 1887
McLean, J. T .....New Philadelphia.....1884
†McMillen, J. W .....Columbus 1878
McMurray, A. B.....Marion 1879
McNeely, J. S.....Hamilton.....1885
*Mack, John.....Shelby 1853
Macready, J.....Monroe 1885
Maris, Clarence.....Columbus 1887
Marquart, O. M .....Osborn 1891
Marshall, S. A.....Cincinnati 1889
Martin, J. T.....Green Camp 1884
Matthews, R. C .....Columbus 1888
Mattoon, F. N .....Plain City 1881
Merriam, E. D .....Conneaut 1884
Merrill, A. E.....Vermillion 1876
Merriman, D. J.....Painesville.....1884
Merz, Charles H .....Sandusky.....1891
Metzler, A. S .....Baltic 1874
Miller, A. T.....Shanesville 1890
Miller, C. A.....Carthage 1871
Mills, C. H .....Toledo 1888
Mills, J. T.....Jersey 1878

Millikin, B. L.	Cleveland	1885
Millikin, Dan	Hamilton	1879
Moore, L. G.	Kinsman	1878
Morehouse, G. W.	Sparta	1890
*Morris, Jonathan	Ironton	1853
Morris, Joseph	Columbus Grove	1879
Morrison, E. B.	Marietta	1890
Mory, D. B.	Wilmington	1884
Mory, J. J.	St. Henry's	1871
Mosgrove, James M.	Urbana	1884
*Mounts, J. L.	Morrow	1860
Murbach, A. J	Archbold	1874
Murphy, J. A.	Cincinnati	1872
Murray, L. S.	Medina	1870
Myers, I. A.	Shelby	1881
Nash, E. K.	Montrose	1886
Nau, J. G.	Carroll	1878
Neil, Alexander	Columbus	1868
Nelson, H. W.	Steubenville	1878
Norris, T. B	Alton	1882
North, John	Toledo	1891
Noyes, H. J.	McConnellsville	1888
Nunemaker, H. B.	Philadelphia In. Asy	1875
Ogan, Ambrose	Sedalia	1884
Ousley, J. B	Jacksonburg	1871
Overly, J. E.	Columbus	1888
Pake, J. H.	Middleport	1890
Palmer, C. D	Cincinnati	1871
Parker, Charles B.	Cleveland	1886
Patterson, James W	Olena	1885
*Patterson, A. V.	Mansfield	1861
*Pearce, E.	Steubenville	1859
Pearce, H. C.	Urbana	1867
Pearce, Henry M.	Urbana	1890
Peck, G. S.	Youngstown	1891
Pennell, W. W.	Fredericktown	1884
Peter, Robert	Canal Dover	1[illegible]

Pettitt, R. R	Dayton	1885
Pomerene, Joel	Mt. Hope	1889
Pomerene, P. P	Berlin	1870
Pomeroy, F. S	Mountville	1884
Pomeroy, O	Chardon	1869
Pope, F. F. H	Dalton	1885
Potter, Nathaniel	Adelphi	1890
Powell, W. S	Defiance	1881
*Price, Joseph	Randolph	1852
Quinn, A. T	Wilmington	1881
Quirk, H. W	Burton	1889
Rankin, T. W	Kirkersville	1881
Ranney, S. W	Hope	1878
Ransohoff, Joseph	Cincinnati	1879
*Reamy, T. A	Cincinnati	1855
Reed, C. A. L	Cincinnati	1881
Reed, Calvin H	Toledo	1873
Reed, R. C. S	Stockton	1867
Reed, R. Harvey	Mansfield	1884
*Reeve, J. C	Dayton	1861
Reinhart, J. C	Toledo	1887
Rheinfrank, J. H	Perrysburg	1886
Rhu, Auguste	Marion	1886
Richardson, A. B	Cincinnati	1891
Ricketts, B. M	Cincinnati	1884
Ricketts, E. Irvin	Cincinnati	1882
†Ridenour, A. W	Massillon	1886
Ridgway, N. B	Galion	1878
Riggs, J. M	Bryan	1873
*Robison, J. D	Wooster	1856
Rodebaugh, H. A	Marysville	1890
*Rodgers, J. H	Springfield	1859
Roebuck, D. Y	Dalton	1886
Rogers, J. H	Louisville	1889
Rosenwasser, M	Cleveland	1880
Row, G. T	Circleville	1881
Rowles, W. T	Columbus	1888

Russell, E. S.	Tuscarawas	1880
Ryan, George W	Cincinnati	1884
Sager, J.	Celina	1882
Santee, Leon B.	Marlboro	1885
Sattler, Robert.	Cincinnati	1872
Saxton, O. H.	Washington C. H	1884
Schiller, A. W	Greenford	1890
Schueller, J. B.	Columbus	1869
Schnetzler, H. M.	Toledo	1874
Schwan, F. W	Poplar	1880
†Scott, A. C.	Bladenburg	1884
Scott, B. B.	Mt. Vernon	1884
Scott, R. E	Bowerston	1887
Scott, W. A	Swanton	1878
*Scott, W. J.	Cleveland	1855
Scott, Xenophon C	Cleveland	1874
*Scoville, S. S.	Lebanon	1859
Seeley, W. W.	Cincinnati	1871
Shaffner, Jno. F.	Willshire	1887
Shallioll, R	Winesburg	1891
Sharp, B. M.	Sidney	1885
Sharp, H. J.	London	1882
Sheldon, S. B.	Five Mile	1882
Sherrard, A. C.	Oakwood	1887
Silver, D. R	Sidney	1870
*Sinnett, E.	Granville	1850
Sisler, L. E	Clinton	1887
Skinner, S. W.	Auburndale	1881
Slocum, C. E.	Defiance	1874
Smith, C. N.	Toledo	1887
Smith, D. B	Cleveland	1870
Smith, J. M.	New Philadelphia	1884
Smith, William G.	Ravenna	1888
Snodgrass, J.	Kenton	1878
*Snyder, D. J.	Scio	1850
Snyder, S. P.	New Bedford	1882
Souders, Samuel	Dean	1879

*Southard, J. M	Marysville	1860
Southard, J. Q	Marysville	1882
Spence, J. A	New Philadelphia	1888
Spencer, E. R	Doylestown	1885
Sperry, W	Tallmadge	1886
Spooner, H. K	Republic	1880
Stamm, M	Fremont	1891
Stanley, E	Sandusky	1870
Stansell, J. A	Forest	1866
Storch, A. B	Milan	1880
Stark, W	Cincinnati	1871
Stein, G. S	Columbus	1888
Steiner, D. W	Lima	1890
Stephens, Alfred H	Eaton	1884
*Stevens, E. B	Lebanon	1853
Stevens, Edward S	Cincinnati	1886
Stevens, M. B	Defiance	1887
Stewart, T. H	Church Hill	1877
Stewart, W. D	Toledo	1887
*Stillwell, Thomas	Fremont	1857
*Stockstill, J. N	New Carlisle	1857
Storey, William	Castalia	1884
*Sweney, R. L	Marion	1859
Szendery, L	Sandusky	1887
Tangeman, C. W	Cincinnati	1888
Taylor, H. Longstreet	Cincinnati	1888
Taylor, W. H	Cincinnati	1874
Telfair, Geo. M	Bloomington	1884
Teters, B. F	Berlin	1886
Thomas, F. W	Marion	1884
Thompson, A. W	Circleville	1890
Thorn, E. I	Yellow Springs	1878
Thorn, S. S	Toledo	1873
Thrasher, A. B	Cincinnati	1876
Tichenor, E. J	Lebanon	1869
Titus, A	Portsmouth	1888
Tobey, H. A	Toledo	1878

Tressel, J. H	Alliance	1889
Trimble, R. T	New Vienna	1881
Tuckerman, L. B	Cleveland	1891
Upson, H. S	Cleveland	1888
Vail, J. B	Lima	1885
Vance, R. A	Cleveland	1881
Vandervort, J. W	Harveysburg	1864
*Vannata, E	Fultonham	1855
Van Pelt, C. L	Toledo	1887
Van Winkle, N. B	Blanchester	1885
Vigor, F. A	New Dover	1884
Von Klein, C. H	Cleveland	1891
Wagenhals, F	Columbus	1885
Waggoner, Joseph	Ravenna	1870
Walker, A. B	Canton	1882
Ward, C. L	Cridersville	1891
Ward, C. S	Warren	1875
Ward, W. A	Conneaut	1880
Warner, Frank	Columbus	1884
Weaver, J. M	Dayton	1865
Webster, E. W	Kingsville	1876
Webster, J. V	Flushing	1890
Weed, T. A	Cleveland	1880
Weeks, O. W	Marion	1884
Welch, A. J	Mt. Sterling	1890
West, T. J	Tiffin	1875
White, G. M	Columbus	1888
White, Horace W	Delaware	1887
Whittaker, J. T	Cincinnati	1871
Wilkins, John A	Delta	1874
Williams, B. O	Martin's Ferry	1891
Williams, J. L	Delaware	1878
Williams, T. B	Cleveland	1864
Williamson, M. S	Findlay	1891
Williard, G. P	Tiffin	1875
Willson, W. H	Greenfield	1865
Wilson, D. C	Ironton	1872

Wilson, E. J	Columbus	1884
Wilson, J. L	Greenfield	1871
Wire, G. W	Wilmington	1885
Wirt, W. E	Cleveland	1891
Wirth, R	Columbus	1886
Wise, D. B	Mt. Eaton	1881
Wood, J. S	Collinwood	1880
Woodbridge, John E	Youngstown	1886
Woodruff, L	Alton	1867
*Woods, D. B	Warren	1852
*Woods, J. T	Toledo	1862
Wright, John W	Columbus	1885
Wright, S. St. J	Tallmadge	1880
Wright, T. B	Circleville	1890
Wright, T. M	Troy	1891
Wylie, A. N	Ripley	1866
Wylie, J. L	Ripley	1872
Yost, W. N	Mt. Blanchard	1879
Ziegler, H. H	Ayersville	1884
Zenner, Philip	Cincinnati	1882
Zopfi, W. J	Findlay	1891

NON-RESIDENT PERMANENT MEMBERS.

Campbell, W. A..... .....Colorado Springs, Col...1888
Koogler, M. A..... .....Eldorado, Kansas..... 1884
Ludington, H..... .... Omaha, Neb..... ...1871
Martin, G..... .....Adrian, Mich..... 1877

HONORARY AND CORRESPONDING MEMBERS.

Professor Ferdinand Hebra,* Vienna, Austria.
Professor Isidor Neumann, Vienna, Austria.
Professor Herman Wiederhofer, Vienna, Austria.
Professor Carl Sigmund,* Vienna, Austria.
Professor Herman Zeissl,* Vienna, Austria.
Professor Ed. C. Seaton, * Medical Inspector of the Privy Council, London, England.
Professor M. Jules Guerin,* Member Imperial Academy of Medicine, Paris, France.

HONORARY MEMBERS.

*Awl, W. M.	Columbus	1872
*Bailey, M. J	New York City	1852
*Baron, P.	Ravenna	1870
Bartholow, Roberts	Philadelphia, Pa	1879
Beeman, P.	Sidney	1872
Blackburn, J. C.	Covington, Ky	1857
*Boerstler, G. W.	Lancaster	1865
*Brodie, William	Detroit, Mich	1875
*Brower, J. H.	Lawrenceburg, Ind	1854
*Childs, H. H.	Pittsfield, Mass.	1848
*Conant, H.	Maumee	1874
*Dalton, N.	Mineral Point, Wis	1871
*Gundry, Richard	Catonsville, Md	1879
*Guthrie, C. B.	New York City	1849
Harding, M. H.	Lawrenceburg, Ind	1854
Haughton, R. E.	Richmond, Ind	1863
*Hayes, I. I.	Philadelphia, Pa	1857
*Hempstead, G. S. B.	Portsmouth	1872
Hibberd, James F.	Richmond, Ind	1871
Hill, E. L.	Oxford	1856
*Hunt, S. P.	Richmond, Ind	1866
Jenks, E. W.	Detroit, Mich	1875
Kerr, John	Canton, China	1875
*Kirtland, Jared P.	Cleveland	1874
Kitchen, J. M.	Indianapolis, Ind	1866
*Knight, J	New Haven, Conn	1849

*Deceased.

*Lee, Charles A	New York City	1848
Lyons, W. B	Huntington, Ind	1866
*McIlvaine, R. R	New York City	1871
*Moe, L. W	Ottawa	1876
*Mussey, R. D	Cincinnati	1865
Noyes, J. F	Detroit, Mich	1875
*Parker, Willard	New York City	1852
Safford, E. D	Parkersburg, W. Va	1870
Senn, N	Milwaukee, Wis	1890
Steele, H. K	Denver, Colo	1872
Stevenson, J. M	Pittsburgh, Pa	1866
*Sutton, George	Aurora, Ind	1854
Sutton, R. S	Pittsburgh, Pa	1884
*Thompson, Robert	Columbus	1865
*Trevitt, William	Columbus	1858
*Tully, William	New Haven, Conn	1849
Vaughan, Victor C	Ann Arbor, Mich	1889

*Deceased.

ADDENDUM

TO THE ARTICLE ON A. C. E. MIXTURE.

Being anxious to record all deaths reported by this mixture, this method is taken to add one fatal case which escaped my attention. It is reported in the British Medical Journal, July 19, 1890, page 185. The patient was a female, aged thirty-six years. My absence from the country during the summer of 1890 will account for the omission.

J. C. REEVE.

CORRIGENDA.

On page seventy-six, thirteenth line, for *one-half* read *one-fifth*, and in fourteenth line for 50 *per cent.* read 20 *per cent.*

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CONSTITUTION AND BY-LAWS

OF THE

OHIO STATE MEDICAL SOCIETY

ADOPTED AT COLUMBUS, OHIO, JUNE 14, 1882.

ARTICLE I.

TITLE OF THE SOCIETY.

The name of this Association shall be "THE OHIO STATE MEDICAL SOCIETY."

ARTICLE II.

OBJECTS OF THE SOCIETY.

The objects of this society shall be the advancement of medical knowledge; the elevation of professional character; the protection of the professional interests of its members; the extension of the bounds of medical science; and the promotion of all measures adapted to the relief of the suffering, the improvement of the health, and the protection of the lives of the community.

ARTICLE III.

MEMBERS OF THE SOCIETY.

SECTION 1. The society shall consist of Delegates, Permanent Members, and Honorary Members.

SEC. 2. Delegates shall receive their appointment,

from the county, or other local, Societies: *Provided*, however, that the officers of these Societies and the Chairmen of all committees who have reports to present, shall be delegates *ex-officio*.

SEC. 3. Delegates from other States and foreign Societies, before admission to a seat in this Society, shall produce a certificate of delegation, signed by the President or Secretary of their Society.

SEC. 4. Delegates from Auxiliary Societies, before admission to a seat in this Society, shall produce a certificate of delegation, signed by the President or Secretary of their Society, and shall, after having complied with the requirements or provisions of Sec. 5, Art. III., become Permanent Members of the Society.

SEC. 5. Permanent Members shall be regular practitioners of medicine in the State of Ohio, shall be graduates of a medical college in good standing, shall be members of a county or district Society where they reside, and their election shall require a majority vote at a regular meeting, their eligibility having been previously reported upon favorably by the Committee on Admissions. Every applicant for permanent membership shall deposit with the Treasurer the sum of $3.00 as a membership fee, and also the dues for the current year (which sums shall be returned to him if his application is rejected). Permanent members alone shall transact the legislative business of the Society. Permanent members who may remove from this State shall, upon their request, be styled Non-resident Permanent Members, and shall, without payment of dues, be entitled to all the rights and privileges of Honorary Members. Non-payment of dues for three years shall work a forfeiture of membership.

SEC. 6. The Society may elect eminent physicians as Honorary Members. Those elected in any year shall not exceed six.

SEC. 7. Honorary Members, and invited guests, shall have the privileges of a seat at the meetings, of present-

ing papers, and of taking part in the discussions, but they shall not vote on any question, nor be eligible to any office. Papers read by them require a special vote, by the Society, of reference to the Committee on Publication, in order to appear in the published Transactions.

SEC. 8. The names of all Permanent Members in good standing, and of all Honorary Members, shall be published annually in the Transactions.

SEC. 9. [Adopted1889.] Any member of thirty years' standing, who has paid dues continuously, shall be declared a permanent member without further payment of dues.

SEC. 10. [Adopted 1891.] All members in good standing in county and district medical societies shall be permanent members of this society, and shall be entitled to all its rights and privileges, including a copy of the annual "Transactions;" excepting that the business of the session shall be transacted by delegate members: *provided*, that all members of the Society up to the present date shall be entitled to vote.

ARTICLE IV.

AUXILIARY SOCIETIES.

SECTION 1. Any county, or other local medical Society, whose constitution embraces the objects of this Constitution and the Code of Ethics of the American Medical Association, may, upon application, become auxiliary to this Society, and shall be entitled to one delegate for every five members, and one for every additional fraction of more than half that number.

SEC. 2. Every auxiliary Society shall enforce the observance by its members of the Code of Ethics adopted by this Society; and shall be authorzied to censure, suspend, or expel any member convicted of violating its provisions.

SEC. 3. Any member of an auxiliary Society who shall feel aggrieved at the action of such Society in en-

forcing discipline among the members, shall have a right to appeal to this Society.

SEC. 4. The Secretary of each auxiliary society, not less than one month preceding each annual meeting of this Society, shall make a full and correct catalogue, with post-office address, of its officers and members in good standing at that time, together with a certified list of its Delegates, and transmit the same at once to the Secretary of this Society. If the name of a member is omitted from this catalogue that was contained in the last preceding catalogue, the Secretary must explain the omission by stating whether the member, whose name is omitted, is dead, has withdrawn, has removed, been expelled, or suspended. Any auxiliary Society that fails to comply with this requirement shall be held to be in contempt, and none of its members shall be allowed to participate in the business of this Society, or be entitled to any of its rights or privileges, until such Society shall have purged itself of the contempt.

SEC. 5. In case of the death of any of its members, who are also members of this Society, it shall be the duty of the Secretary of each auxiliary Society to transmit to the Secretary of this Society a suitable obituary notice for publication.

ARTICLE V.

OFFICERS.

SECTION 1. The officers of this Society shall be a President, four Vice Presidents, Secretary, Assistant Secretary, and a Treasurer and Librarian.

SEC. 2. The officers shall be elected by a ballot vote of a majority of all the members present, shall enter upon their duties at the close of the meeting at which they were elected, and shall serve one year, or until their successors are elected.

SEC. 3. All vacancies in office, occurring in the interim

of the meetings, shall be filled by appointment of the President.

ARTICLE VI.

DUTIES OF OFFICERS.

SECTION 1. The President shall preside over the meetings, preserve order, call meetings when in his judgment the interest of the Society may require, deliver an address, and perform such other duties as custom and parliamentary usage may require.

SEC. 2. The Vice-Presidents shall assist the President in the performance of his duties, and, in his absence, preside over the meetings, in the order of their seniority.

SEC. 3. The Secretary shall keep correct minutes of the proceedings of the Society, and prepare and furnish the same to the Committee on Publication for publication in the Transactions, notify Committees and Society Delegates of their appointment, take charge of all manuscripts, papers, etc., read at the various meetings, and transfer the same to the Committee on Publication for publication; give due notice of the annual meetings, conduct all business correspondence, give official notice to all auxiliary Societies of resolutions, etc., passed by this Society that have reference to the said Societies, and also notify them of the time for making their reports to this Society, and perform all other duties which common usage of other corporate or organized bodies may require. His salary shall be $125 per annum.

SEC. 4. The Assistant Secretary shall assist the Secretary in the performance of his duties as the latter may require, and act for him in his absence. His traveling and hotel expenses shall be borne by the Society.

SEC. 5. The Treasurer and Librarian shall receive all moneys due the Society, pay all bills countersigned by the Secretary, keep a correct, itemized account of all receipts and expenditures, with all vouchers, and make a full re-

port of the same at each annual meeting. He shall have charge of all the books, manuscripts (not specially belonging to the Secretary), instruments, specimens, preparations, and other scientific property belonging to the Society, keeping a complete catalogue of the same, and report the condition of this department at the annual meeting. He shall give bond for the faithful performance of his duties in the sum of twice the amount of money in his possession, with two sureties, said bond to be deposited with the Secretary. His salary shall be $125 per annum.

SEC. 6. The officers shall deliver all records, books, funds and other property belonging to their several offices, to their successors, when the latter shall enter upon the discharge of their respective duties.

ARTICLE VII.

COMMITTEES.

The Committees of this Society shall be Standing and Temporary. Standing Committees are those whose duties continue from year to year. They shall each consist of five members, and, unless otherwise specified, shall be elected by the Society, one member being elected each year, except the first year, when one member shall be elected for five years, one for four, one for three, one for two, and one for one year. The Standing Committees shall be: Committees on Arrangements, Finance, Admissions and Medical Societies, Ethics, Publication, and Medical Legislation. Temporary Committees are those whose functions cease with the meeting at which they are appointed. They shall be appointed by the President, as the necessity for their existence arises.

ARTICLE VIII.

STANDING COMMITTEES AND THEIR DUTIES.

SECTION 1. The Committee on Arrangements shall, if no sufficient reason prevent, be mainly composed of

members at the place where the next annual meeting is to be held. It shall provide suitable rooms and accommodations for the meeting, and, in all matters not otherwise provided for, superintend and protect the general interests of the Society. This Committee shall be appointed annually by the President.

SEC. 2. The Committee on Finance shall superintend the monetary affairs of the Society, inspect and audit all bills and accounts of the Treasurer, examine and determine the amount, and make such assessment, by a *pro rata* tax upon the Permanent Members, as may be necessary for incidental expenses, and perform such other duties as may devolve upon it.

SEC. 3. The Committee on Admissions and Medical Societies shall examine and pass upon the qualifications of Delegates, and the applications for admission of auxiliary Societies, and perform such other duties as may devolve upon it.

SEC. 4. The Committee on Ethics shall examine and report for the action of the Society, all cases of appeal from the auxiliary Societies, and such other matters as may be referred to it by the Society. If any member shall be charged, in writing, with any violation of the provisions of this Constitution and By-Laws, or with unprofessional conduct, a copy of such charge shall be furnished to him by the Secretary, and himself and accuser cited to appear, when the Committee shall proceed to hear the case, reserving its decision to be reported to the Society; except that any case may be referred to an auxiliary Society for adjudication, and from the decision of the auxiliary Society an appeal may be carried to this Society. Reports from this Committee, when unanimous, shall be acted upon without debate.

SEC. 5. The Committee on Publication, of which the President and Secretary shall be members *ex-officio*, shall examine all papers presented to the Society, and have printed in the Transactions such as it deems worthy of publication.

SEC. 6. The Committee on Legislation shall watch the course of State legislation on medical subjects, and take charge of such matters as shall be referred to it by the Society. All proposed legislation offered by any member of this Society, and all resolutions touching medical legislation, shall be referred to this committee, which shall make a report thereon to this Society as soon as possible. It shall also, at each annual meeting, report to the Society any laws relating to medical matters that may have been passed within the year.

SEC. 7. The Standing Committees shall keep regular minutes of their proceedings, and furnish an authenticated copy thereof, to be deposited with the Librarian.

ARTICLE IX.

DELEGATES.

SECTION 1. The Delegates appointed by this Society to the American Medical Association or to other Societies, shall report briefly, in writing, through their Chairman, such items of general interest as may have claimed the attention of the bodies to which they were respectively delegated.

ARTICLE X.

MEETINGS.

SECTION 1. The meetings of this Society shall be annual, and shall occur at such time and place as the Society may appoint: *Provided*, that special meetings may be called at any time by the President, and shall be so called by him on the written request of twenty-five members. Due notice of all the meetings shall be given by the Secretary, and in case of a special meeting the object for calling said meeting shall be distinctly set forth in the notice.

SEC. 2. Fifteen Permanent Members shall constitute a quorum for the transaction of business.

ARTICLE XI.

CODE OF ETHICS.

The Society adopts, as a part of its regulations, the Code of Ethics of the American Medical Association.

ARTICLE XII.

AMENDMENTS.

Every proposal for amending the Constitution shall be made in writing; and, if such amendment receive a majority of four-fifths of the Permanent Members present, it shall be adopted; but otherwise, the amendment shall lie over until the next meeting, when it may be adopted by a majority of two-thirds of the Permanent Members present.

ARTICLE XIII.

Nothing in this Constitution shall be so construed as to deprive any one now a member of this Society, of any rights or privileges held by him before the adoption of this Constitution, unless voluntarily surrendered by him.

BY-LAWS.

I.

ORDER OF BUSINESS.

1. The President, or, in his absence, one of the Vice-Presidents, shall call to order; or, in case of the absence of all these officers, a chairman shall be appointed *pro tempore.*

2. Reading of minutes. Report of the Committee of Arrangements (on first day).

3. President's address (afternoon of second day).

4. Election of officers (afternoon of second day).

5. Selection of a place for the next meeting (afternoon of second day).

6. Business which requires early consideration.

7. Reports of Standing Committees.

8. Reports from Delegates to the American Medical Association, and other Societies.

9. Written communications on medical subjects (papers).

10. Oral communications.

11. New business.

12. Unfinished and miscellaneous business.

13. Adjournment.

II.

In filling blanks, the longest time and the largest sum shall be first put.

III.

Any three Permanent Members may call for the ayes and noes.

IV.

All questions of order, not especially provided for, shall be decided according to Cushing's Manual of Parliamentary Practice.

V.

Not to exceed twenty minutes shall be allowed for the reading of any paper.

VI.

No paper shall occupy over thirty pages of the Transactions.

VII.

All reports, papers and addresses presented to this Society by its members are the property of the Society, and shall be referred, without motion and without recommendation, to the appropriate committee; and it shall be considered a misdemeanor for the author of any such report or paper to permit the same to be published except as authorized by special vote of the committee or the Society.

VIII.

No speaker shall be allowed to speak a second time, on the same subject, without the consent of the Society.

IX.

The time allowed each speaker shall not exceed ten minutes, except by general consent.

X.

The motion to adjourn, to lie on the table, or for the previous question, shall be decided without debate.

XI.

The motion for the previous question, if decided in the negative, shall have no effect on the main question.